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THE U.S. DEPARTMENT OF HEALTH AND HUMAN SERVICES

AGENCY FOR TOXIC SUBSTANCES AND DISEASE REGISTRY

convenes the

EXPERT PANEL MEETING

Analysis and Historical Reconstruction of Groundwater Resources and Distribution of Drinking Water at Hadnot Point, Holcomb Boulevard and Vicinity, U.S. Marine Corps Base, Camp Lejeune, North Carolina

APRIL 29, 2009

The verbatim transcript of the Expert Panel Meeting held at the ATSDR, Chamblee Building 106, Conference Room A, Atlanta, Georgia, on Apr. 29, 2009.

ORIGINAL

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C O N T E N T S

April 29, 2009

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EXPERT PANEL

Analysis and Historical Reconstruction of Groundwater Resources and Distribution of Drinking Water at Hadnot Point and Holcomb Boulevard and Vicinity, U.S. Marine Corps Base, Camp Lejeune, North Carolina.

PANEL MEMBERS

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Panel Chair, Environmental Engineering & Public Health Consultant

Cincinnati, Ohio

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Associate Chairman and Professor, Department of Epidemiology, Boston University School of Public Health

Boston, Massachusetts

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Columbus, Ohio

Richard Clapp, DSc, MPH

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P A R T I C I P A N T S

(alphabetically)

ANDERSON, BARBARA, ATSDR
ARAL, M., GT
ASCHENGRAU, ANN, BOSTON UNIV.
ASHTON, BRYNN, USMC
BAIR, EDWIN, S., OHIO STATE U.
~~BELGIN~~, [BELJIN -ed.] MILOVAN, SHAW, INC.
BURTON, THOMAS, USMC
CIBULAS, WILLIAM, ATSDR
CLAPP, RICHARD, BUSPH
CLARK, ROBERT, INDEPENDENT CONSULTANT
CLARK, MAJ. VERN, SELF
DOUGHERTY, DAVE, SUBTERRANEAN RESEARCH
ENSMINGER, JERRY, CAP
FAYE, ROBERT E., CONSULTANT
GAMACHE, C., USMC
GOVINDARAJU, RAO S., PURDUE UNIV.
GRAYMAN, WALTER M., GRAYMAN CONSULTING
GREEN, STEVEN RAY, SRG & ASSCS.
HARDING, BEN, AMEC
HARTSOE, JOEL, USMC
HILL, MARY C., USGS
KONIKOW, LEONARD, USGS
MASLIA, MORRIS, ATSDR/DHAC
PARTAIN, MIKE, CAMP LEJEUNE CAP
POMMERENK, PETER, INDEPENDENT CONSULTANT
ROSS, RANDALL, USEPA
RUCKART, PERRI, ATSDR
SAUTNER, JASON, ATSDR
SCOTT, CHERYL, EPA
SIMMONS, MARY ANN, NAVY
~~SUAVEZ~~ [SUÁREZ-SOTO -ed.], ~~RENE~~ [RENÉ -ed.] J., ATSDR/DHAC WADDILL, DAN, NAVFAC
WARTENBERG, DAN, UMDNJ
WILLIAMS, SCOTT, USMC

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Glossary of Acronyms and Abbreviations

ASCE	American Society of Civil Engineers
AST	above ground storage tank
ATSDR	Agency for Toxic Substances and Disease Registry
AWWA	American Water Works Association
BTEX	benzene, toluene, ethylbenzene, and xylenes
CAP	community assistance panel
CD-ROM	compact disc, read-only-memory
CERCLA	Comprehensive Environmental Response, Compensation,
and Liability Act	
CI	cast iron
DCE	DCE:
	dichloroethylene
	1,1-
DCE:	1,1-dichloroethylene or 1,1-dichloroethene
	1,2-
DCE:	1,2-dichloroethylene or 1,2-dichloroethene
	1,2-
cDCE:	*cis*-1,2-dichloroethylene or *cis*-1,2-dichloroethene
	1,2-
tDCE:	*trans*-1,2-dichloroethylene or *trans*-1,2-dichloroethene
DHAC	Division of Health Assessment and Consultation, ATSDR
DOD	U.S. Department of Defense
DON	U.S. Department of Navy
EPANET or EPANET 2	a water-distribution system model developed by the EPA
ERG	Eastern Research Group, Inc.
gal	gallons
gpm	gallons per minute
HPIA	Hadnot Point Industrial Area
HUF	hydrologic unit flow
IRP	installation restoration program
LGR	local-grid refinement
MESL	Multimedia Environmental Simulations Laboratory, Georgia Institute of Technology
MGD	million gallons per day
μg/L	micrograms per liter
MODFLOW	a three-dimensional groundwater flow model developed by the U.S. Geological Survey
MODPATH	a particle-tracking model developed by the U.S. Geological Survey that computes three-dimensional pathlines and particle arrival times at pumping wells based on the advective flow output of MODFLOW
MT3DMS	a three-dimensional mass transport, multispecies model developed by C. Zheng and P. Wang on behalf of the

	U.S. Army Engineer Research and Development Center, Vicksburg, Mississippi
NAVFAC	Naval Facilities Engineering Command
NCEH	National Center for Environmental Health, U.S. Centers for Disease Control and Prevention
NTD	neural tube defect
PCE	tetrachloroethylene, tetrachlorethene, PERC® or PERK®
PEST	a model-independent parameter estimation and uncertainty analysis tool developed by Watermark Numerical Computing
ppb	parts per billion
PVC	polyvinyl chloridc
SGA	small for gestational age
Surfer®	a software program used for mapping contaminant
plumes in groundwater	
TCE	trichloroethylene, 1,1,2-trichloroethene, or 1,1,2-
trichloroethylene	
TechFlowMP	a three-dimensional multiphase multispecies contaminant fate and transport analysis software for subsurface systems developed at the Multimedia Environmental Simulations Laboratory (MESL) Research Center at Georgia Tech
TTHM	total trihalomethane
USEPA	U.S. Environmental Protection Agency
USMC	U.S. Marine Corps
USGS	U.S. Geological Survey
USPHS	U.S. Public Health Service
UST	underground storage tank
VC	vinyl chloride
VOC	volatile organic compound
WTP	water treatment plant

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P R O C E E D I N G S

(8:15 a.m.)

HOUSEKEEPING RULES

MR. MASLIA: I'd like to welcome everybody and thank especially the experts on the panel for coming to this two-day panel meeting and providing input to the Agency and to those working on the Camp Lejeune Health Study. It means a lot of us for you to provide us with your time and input and appreciate your pre-meeting comments.

And I'll just go over some house rules. You came in at the Visitor's Center. This is for lack of a better word an official federal facility or compound. So you are prisoners of Building 106, and my name I think is on all of your visitors' badges. I'm not sure if you want to claim that or not, but if you walk outside the building I'm sure I'll hear about it. So with that we'd like to ask that all of your activities remain in Building 106 if at all possible.

There is a cafeteria. Some of you passed in front of it as you came in, and

there's lunch there. While we don't officially have reserved tables, we have set aside a row of about 25 or 30 seats that have reserved signs for the expert panel at the end of the cafeteria by the outside atrium as you walk past the cashiers all the way to the end. So if y'all want to sit together, that's fine. We'll make that possible.

And also, there are vending machines to my right outside the room here. Also, as I said, due to security it's advisable not to leave the building. We can't do it without one of us or ATSDR person and but for this evening or whatever, there's all sorts of fast food, ethnic restaurants up and down Buford Highway, which is a strip you came down, the seven-lane strip you came down this morning if you were awake to watch much of the scenery. Snack rooms as I said. The restrooms are to my left a couple of doors down.

We've got a number of people helping us. I just want to -- I'm sure I've left somebody off, so just let me know. But Liz ~~Burlsen*~~ [Bertelsen -ed.], who is from ERG, and has been in contact with most of our

expert panel members. Jerome Cater*, Chris Fletcher*, Cathy Hemphill* in the back who brought us some coffee, Rachel Rogers* and Jane Tsu*. I don't think she's here.

Miscellaneous items: This is a sensor mike system, so you push the red button twice, and the red ring will come on around the top of the mike, and please speak into the mike. On the long tables here we've got four for five people, so share. You on the short table, y'all each have your own mike.

Please state your name for the first time -- we've got a court reporter -- when you speak into the mike or during the public session, when people come up, please state your name and affiliation.

This meeting is being audio taped, streamed live to the web and videotaped. It is a public meeting. As I said there's a court reporter recording everything, and that'll be part of the meeting report just like -- for those of you who were in the first expert panel meeting in 2005, the report that came out had two CDs with the verbatim transcripts. The same thing will happen here.

You'll, of course, get an opportunity to correct that or see a draft report obviously before it goes final to correct any information.

Turn off your cell phones to silence or vibrate and please no sidebars because it's difficult for the court reporter to record what you're speaking about on the side, and it may prove very important to us at ATSDR for those comments. So we'd like to hear it in public.

And that is it for housekeeping rules. Any questions?

(no response)

MR. MASLIA: At this time I'll bring up Dr. Sinks.

OPENING REMARKS AND INTRODUCTION OF CHAIR

DR. SINKS: Good morning everybody. My name is Tom Sinks. I'm the Deputy Director for both the National Center for Environmental Health and the Agency for Toxic Substances and Disease Registries, a long title. And I just wanted to welcome you here today. I am not an engineer. I am not an engineer. I'm an epidemiologist.

I have two of my mentors during my graduate school were actually converted engineers into epidemiologists of all things, and it may be why I got into the Environmental Health area. Because a lot of epidemiology is focused on physicians who become epidemiologists, the people from the health side who then go on to look at health issues.

And it's very important, at least in Environmental Epidemiology, for people on the exposure side to become involved in epidemiology because of an appreciation of how important it is to get exposure right. And if you have any appreciation for epidemiology, misclassification of either exposure or disease, is critical to the quality of your work.

And in general, if it's unbiased misclassification, it will always drive you towards not finding associations. So we are very, very concerned in Environmental Epidemiology that we get exposure right; hence, this is why we have you.

It's not unusual in situations where you have Environmental Epidemiology you're

trying to look back over time that you have precious little information about exposure. And somehow you have to go back and try to figure out as accurately as possible what people were exposed to when you really don't have the information you would like to have, which is, gee, I wish I had some monitors on the tap water -- in this case, Hadnot Point from 1950 until 1985 -- so I knew exactly what these people were, and, gee, I wish I knew exactly how much they were drinking and how often they were showering, da-da-da da-da.

We don't have that information. We'd love to have it, but what we're going to do is use fairly sophisticated techniques to try to get back to the best information we can so we can do a good job with our epidemiology.

A couple things I want to say to you. First of all, I always appreciate Morris because he does such a great job. He wrote my opening remarks, and I'll pass these around for you if you'd like to see them because I don't plan to use them, but thank you, Morris. I'm sure they would have come out much more gracious than I will in person.

I want to make a couple of comments to you. For us, Monday -- no, Tuesday through Thursday is of all things a Camp Lejeune marathon. Yesterday we had our community advisory committee -- no, Community Assistance Panel, thank you, our CAP. Some of those members are here today. And the next two days we have this panel.

And one thing that I am very pleased with in terms of this project is the amount of openness and transparency that we're trying to put into this project. I think we can always try to do more, and if there are ways we can do more, we're interested in hearing that. But that's something that I think is somewhat unique about ATSDR. I'm very proud of it, and I think we are trying to do the best job possible on that.

Also, on this project and many of our projects we're very interested in not doing these solely intramurally. We're very interested in critical comment. Not just comment that says, hey, that's fine. Keep going. But a critical comment that says this is where I think you could do better.

Now in terms of being a scientist in this program and a supervisor, our job is to do exactly that with our staff. And we're not doing that if our staff are not being critical of ourselves all of the time. We should be doing that. We're hoping you will be doing that. You don't have to be too critical, but that's an important role for us.

And in Camp Lejeune, at least since I've been involved with this project, this is the third expert panel that we've held on Camp Lejeune. The first one had to do with seeking some advice from outside experts on additional epidemiologic studies. We had one similar to this on Tarawa Terrace, and this one today on Hadnot Point on exposure modeling.

And of all things, the National Academy of Sciences is writing a very large report we heard on Camp Lejeune. And we heard yesterday that the report that was scheduled to come out next week is now delayed again. So that's another piece of this.

So we're getting quite a lot of that. We will continue to get that. When we issue our reports, we'll put them out as public

comment. We will get more comment then, but that's part of the process.

In terms of this project, I think you're probably very well aware of the charge. And I'll just say maybe simply we want to get the best information we can. Now, at the same time I really don't want to spend five years trying to figure out the best information we can. I really want to make sure we're getting the best information we can; we're doing it in a timely way, and we're proceeding along to get these projects finished.

Because, frankly, when I retire when I'm 70 -- because my youngest is six years old now -- when I retire when I'm 70, I hope I'm no longer in the business of Camp Lejeune. I know it will be something that has great interest to many people, but I hope we can get our projects finished, get the information out that needs to get out and get things done that need to be done at Camp Lejeune.

And so while you're looking at this, and you're scrutinizing this, I hope you recognize that this is not just an exercise in excellence. It's an exercise in an applied

public health approach to an applied problem that people need answers to, and we really want to move ahead and get the best job we can done.

So with that I'll just close, and I hope you liked my opening comments whatever they were. And with that, Morris.

MR. MASLIA: Introduction of panel members.

DR. SINKS: I didn't realize you wanted me to do that, but you did give me this so I will introduce this. Most importantly, Bob Clark is from Cincinnati, Ohio, where I spent six years working for the National Institute of Occupational Safety and Health. I lived in Hyde Park right next to Graeter's Ice Cream. I could walk down there every afternoon, and I gained five to ten pounds.

Bob is a registered engineer and, I believe, a friend to epidemiologists. Currently, an independent environmental engineering and public health consultant. He retired from EPA in 2001. He's worked as environmental engineer at the --

You were a commissioned officer?

DR. CLARK: Right.

DR. SINKS: He was a commissioned officer working in U.S. EPA, which is actually a fairly rare thing. He was Director of the Water Supply and Water Resources Division at EPA from '85 to '99, and was appointed to a senior expert position at the EPA. He's authored or co-authored more than 350 papers and published five books. And I guess I'm going to turn this over to you.

MR. MASLIA: I was remiss in not stating, and I apologize to the experts and the audience. Those who have been in... We originally had James Blumenstock as our Panel Chair, which was on the original, and James, working for ~~ASTO~~ [ASTHO -ed.], got called up Monday morning to head their federal task force on the swine flu.

And so on short notice, Bob Clark has done a number of these panels, and I just want to assure for the record, that neither ATSDR, NCEH or CDC have any financial obligations or association with Bob Clark, and there is no conflict of interest, and we're appreciative of Bob's effort to step in at a moment's notice.

OPENING STATEMENT AND PRESENTATION OF CHARGE

DR. CLARK: Thank you, Morris, and thank you, Tom.

When James couldn't do it, well, they visually scraped the bottom of the barrel and came up with what they could find, and so that's me. So I will be the chairman this morning.

As all of you have been with the government or are with the government or affiliated with the government, you know there's a certain amount of bureaucracy that goes on. And one of the things we have to do, I have to read the charge so that we establish the fact that this is an official government meeting, so I'm going to do that.

This is the expert panel assessing ATSDR's methods and analysis for historical reconstruction of groundwater resources and distribution of drinking water at Hadnot Point, Holcomb Boulevard and vicinity, U.S. Marine Corps Base, Camp Lejeune, North Carolina. The purpose and scope of this expert panel is to assess ATSDR's efforts to model groundwater and water distribution

systems at the U.S. Marine Corps Base, Camp Lejeune, North Carolina.

This work includes data discovery, collection and analysis as well as water modeling activities. To assist the panel members with their assessment, they have been provided with the methods used and results obtained from ATSDR's previous modeling efforts at Camp Lejeune which focus on the area of Tarawa Terrace and vicinity. This panel is specifically charged with considering the appropriateness of ATSDR's approach, methods and time requirements related to water modeling activities.

It is important to understand that the water modeling activities for Hadnot Point, Holcomb Boulevard and vicinity are in the early stages of analysis; hence, the data interpretations and modeling methodology are subject to modifications partly based on input provided by members of this panel.

ATSDR expresses a commitment to weigh questions from the public and to respond to public comments and suggestions in a timely fashion. However, in order for this panel to

complete its work, it must focus exclusively on data discovery and analysis and water modeling issues. Therefore, the panel will only address questions or comments that pertain to data discovery and analysis and water modeling efforts.

For all non-water modeling questions or statements, the public can contact the ATSDR Camp Lejeune Information Hotline at telephone ~~7-7-0-4-8-8-3-5-1-0~~ [770-488-3510 – ed.] or e-mail atsdrcamplej@cdc.gov. So that's the obligatory business that we have to take care of this morning.

One thing I want to be sure is we have a fair and open discussion. I certainly don't want to cut off any discussions or the opportunities for the experts to express their opinions, especially this panel. But we do have a very tight and specific agenda that we're going to have to try to complete. And so I'm going [to –ed.] hold fairly tightly to this so I want to warn you now that if I request that you terminate your discussion or your questions, it's not because I don't want to hear them; it's because we need to meet the

tightness of our deadline. So I'm going to try to hold tightly to the agenda.

If there's additional comments, for example, if the web people, web-streaming people have comments, they can send e-mails into ATSDR to get their questions answered. Anybody here who has questions or feel like there's an issue that has not been well addressed can submit those questions or comments in writing. I think Morris can give you a contact point for that. We want to be sure that we have the maximum input, but we particularly, of course, want to hear from this excellent expert panel.

INTRODUCTION OF PANEL MEMBERS, AFFILIATIONS, AND RELATED EXPERIENCES

Just to give you a little more background on my background, we'll go around the table and introduce ourselves. I spent 41 years with the U.S. Public Health Service and the U.S. EPA, 30 of those years were as a U.S. Public Health Service commissioned officer. So I'm very familiar with some of the uniforms that I see in the room today.

I was detailed to the EPA when it was

created and ~~was,~~ [-ed.]for 14 years of that time, I was Director of the Water Supply and Water Resources Division in Cincinnati. I was actively involved in helping set the standards and develop the technologies that are utilized under the Safe Drinking Water Act for treating the kinds of chemicals we're going to be talking about today, so I'm very interested in this area. I spent three years as a senior scientist and since that time, I retired in 2002, I've been an independent consultant.

So let's go around the room. Randall.

DR. ROSS: My name is Randall Ross. I'm a hydrogeologist at the Robert S. Kerr Environmental Research Center, Ada, Oklahoma, for the U.S. EPA. I've been with EPA 22 years, I guess, at Kerr Lab working for the, what's now called the Applied Research and Technical Support Branch, providing technical assistance to EPA regional offices and hazardous waste sites in all ten regions over that time, mostly in hydrogeology, drilling and groundwater modeling-related activities.

DR. KONIKOW: My name is Lenny Konikow. I'm a research hydrologist, hydrogeologist with

the U.S. Geological Survey in Reston, Virginia. I've been with the USGS for about 37 years, mostly in the research program and have been involved in developing groundwater flow and ~~solutransport~~ [solute-transport -ed.] models and applying them to groundwater contamination problems as well as water supply problems.

DR. GOVINDARAJU: Hello, I am Rao Govindaraju. I'm a professor in the School of Civil Engineering at Purdue University. My area of expertise is in surface and sub-surface flows and contaminant transport. I've been at Purdue for about 12 years now, and before that I was a faculty member in Kansas for five years.

MR. HARDING: I'm Ben Harding. I'm a civil engineer with AMEC Earth and Environmental in Boulder, Colorado, originally trained as what was then called a sanitary engineer, worked in advanced waste treatment for a number of years and then started to practice warm water resources and done a number of reconstructions of fate and transport of contaminants in water distribution systems. And I'm interested in

risk assessment and treatment of uncertainty.

DR. CLAPP: My name is Dick Clapp. I'm an epidemiologist now at Boston University School of Public Health where I've been on the faculty for the last 18 years. Prior to that I worked as Director of the Massachusetts Cancer Registry and was deeply involved with the Woburn Childhood Leukemia Cluster and the water model that was created by a geologist at the University of Massachusetts in Amherst, named Peter Murphy.

And subsequently to that I worked in the consulting company and was hired as a consultant to the Ocean County Health Department in New Jersey where they were concerned about the Toms River exposures from hazardous waste sites that may have affected childhood cancer.

I'm currently a member of the CAP, and I, as a result of that, get paid per diem by ATSDR. I was here yesterday for the CAP meeting, and I've been for the last three years.

DR. POMMERENK: My name is Peter Pommerenk. I'm an environmental engineer. I am currently

an independent consultant and used to consult on various Marine Corps and Navy contracts with Camp Lejeune, working on water treatment projects and water distribution projects.

DR. WARTENBERG: I'm Dan Wartenberg, a professor and Chief of the Division of Environmental Epidemiology at Robert Wood Johnson Medical School. And most of my research is on spatial epidemiology and GIS applications in epidemiology and also on disease clusters. And in 2000 I wrote the epidemiology section of EPA's reassessment of TCE, which I guess is still to move forward in terms of regulation.

DR. BAIR: My name is Edwin Scott Bair. I go by Scott. I'm a faculty member at Ohio State University in the Department of Earth Sciences. I have experienced six years with Stone and Webster Engineering Corporation. I worked with the USGS 16 years as a part-time employee.

And if I have a distinction at this table, it's being the only one who's lived at Camp Lejeune in 1952 when my father was called back into the Marines. My interests are in

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ground water hydrology, fate transport modeling. And one of my Ph.D. students, Maura Metheny, several years ago did a lot of work on the cancer cluster up at Woburn, Massachusetts.

DR. ASCHENGRAU: My name is Ann Aschengrau. I'm an environmental epidemiologist at Boston University School of Public Health. I'm a classically trained epidemiologist, and the area of research that I've been investigating for probably about 15 years now is solvent-contaminated drinking water. The research has been done primarily in the Cape Cod area of Massachusetts, which experienced exposure to tetrachloroethylene through the drinking water supply. I've also been investigating the spatial epidemiology of cancer and other diseases in the Cape Cod area, and happy to be here today.

DR. DOUGHERTY: My name is Dave Dougherty. I'm a consultant ~~on subterranean research~~ [at Subterranean Research -ed.] in Duxbury, Massachusetts. I'm trained as an engineer and my expertise is in groundwater. My career arc has gone from consulting to academia and back

to consulting. I was a faculty member at the University of California Irvine and the University of Vermont. Back to Toms River, my first consulting gig was putting together a 3-D flow and transport at Toms River 25 years ago and has moved on to optimization perimeter estimation and long-term monitoring.

DR. HILL: Hi, my name's Mary Hill. I am a Research Hydrologist with the U.S. Geological Survey and have my educational background is geology and civil engineering. And I have specialized in with groundwater models, specifically integrating data and models, essentially how to do that best, what the uncertainty is, calibration methods, sensitivity analysis methods. And my book, actually a copy of my book is over there. It came out a couple of years ago. And I also, as part of that book, developed a set of guidelines for model calibration. There's a lot of talk about guidelines in this and what to use. Also, some years ago for a Proceedings article, I did a review of existing guidelines for groundwater model development and had submitted those. I don't

know if they're around, but there were some questions about what guidelines might be available so that might be useful. Thank you.

DR. GRAYMAN: Good morning. I'm Walter Grayman. I'm an independent consulting engineer in Cincinnati and have been for the past 25 and-a-half years. My background is in civil and environmental engineering, but for the past, again, about 25 years I've been working in modeling of water distribution systems, hydraulic modeling and working with Bob Clark early in terms of developing water distribution system, water quality models. I did serve as a consultant for ATSDR on the Camp Lejeune work for a few years back when they were first starting it in terms of the field analysis modeling.

DR. CLARK: Well, thank you everybody. I'm sure we have a very highly qualified panel, and I'm looking forward to hearing everybody's comments. I'm sure they're going to be quite pertinent; it's going to be an interesting session, I think.

Morris, you're up next with your staff.

INTRODUCTION OF CAMP LEJEUNE EPIDEMIOLOGICAL STUDY TEAM

INTRODUCTION OF STAKEHOLDERS

MR. MASLIA: At this point Frank and I will introduce the ATSDR Health Studies staff and stakeholders as well.

Frank, I think you're up first so --

DR. BOVE: My name is Frank Bove. I'm a Senior Epidemiologist in the Division of Health Studies at ATSDR, been at ATSDR since 1991, before that with the New Jersey Health Department. And I'm co-PI on this work.

Perri Ruckart is back there. She's also co-PI, and she's an Epidemiologist in the Division of Health Studies. And Carolyn Harris, who's sick today, she's a Public Health Analyst who works on our budgets and contracts with contractors and so on. So that's the epi side of the picture.

INTRODUCTION OF WATER MODELING TEAM

MR. MASLIA: From the water modeling side, the study -- of course, I'm Morris Maslia. I'm a Research Environmental Engineer, and I've been with ATSDR and CDC since 1992, and I also spent almost ten years with the U.S. Geological Survey back in the days when we had

money to do lansa^ [RASA (Regional Aquifer System Analysis) -ed.] studies and water resource we talked about.

Since the first panel, is interesting. We have the Agency has put resources in obtaining additional full-time staff. For those who were on the first panel, remember Jason Sautner was the only other full-time person with me, back there. Since then we've added Barbara Anderson in the back row, and Rene Suarez. And we also have Bob Faye, who's with Eastern Research Group, who was also with us for the first panel. And Dr. Mustafa Aral from the Multi-media Environmental Simulations Lab at Georgia Tech.

And at this point Frank and I would also like to introduce stakeholders, and if we miss anybody, please, if you want to stand up and introduce yourselves, but we have from Camp Lejeune and Marine Corps Headquarters, I see Scott Williams, who has been our point of contact both previously at Camp Lejeune and now at Headquarters. We've got Dan Waddill from the Navy. I see Joel Hartsoe from Camp Lejeune and Brynn Ashton, also, Thomas Burton.

And are there other people from the --

MR. GAMACHE: Chris Gamache.

MR. MASLIA: Chris Gamache, I know I'd miss somebody, welcome.

Then on the CAP -- oh, I'm sorry, I forgot Mary Ann Simmons, forgive me.

DR. BOVE: Mary Ann's also the DOD representative on the Community Assistance Panel. And Mike Partain, back there, is also a community member on the Community Assistance Panel. And Jerry Ensminger walked out just now, but he'll be back, is also on the Community Assistance Panel.

MR. MASLIA: Is there anybody else who -- I know we have a representative from EPA from Cincinnati.

MR. ~~BELGIN~~ [Beljin -ed.]: Milovan ~~Belgin~~ [Beljin -ed.] ^ ~~geologist~~ [hydrogeologist -ed.].

MR. MASLIA: And I've corresponded with him along with Dr. Ross for the expert panel. So welcome everybody. And at this point we're a little ahead of schedule which is good.

SUMMARY OF CURRENT HEALTH STUDY

Frank, let me pull up your and Perri's

presentation, and we'll proceed with the current health study, big picture, from Frank and Perri.

MS. RUCKART: Good morning, Perri Ruckart, ATSDR. Frank and I are just going to briefly describe our current health study at Camp Lejeune for you. We already introduced the project team.

Now, ATSDR has conducted or is in the process of conducting several health studies at the base, and we started by looking at the health effects on children or fetuses because they were seen to be the most vulnerable population on chemical exposures. In 1998 we published a study on adverse pregnancy outcomes. We evaluated potential maternal exposure to drinking water contaminants and the following outcomes: pre-term births, small for gestational age and mean birth weight deficit.

At that time we were only able to use available databases. There was no water modeling. We used electronic birth certificates beginning in 1968, and during 1968 to 1985, when most of the contamination

ended, there were 12,493 singleton live births on the base.

And to assign the exposure we looked at base family housing records and linked those to the mother's address at delivery and usually the father's name. But we could not evaluate birth defects and childhood cancers because we're just relying on information from the birth certificates.

The results of this study showed that exposure to Tarawa Terrace water, which was contaminated with PCE, there was an elevated risk for small for gestational age among infants born to mothers greater than 35 years and mothers with two or more previous fetal losses. As far as the exposure to Hadnot Point water and TCE, there was an elevated risk for SGA only among male infants.

However, going through this water modeling process we discovered new data -- I'm sorry, we discovered that there was exposure misclassification because an area that was previously categorized as unexposed is going to be exposed. So once we have the water modeling results, we're going to go back and

re-analyze the results from the 1998 study.

Now we also have a current case-control study, and I want to point out to you that here at ATSDR we do have peer review of our study protocols and the final study reports. I just want to mention that all of our work here has been peer reviewed.

So the current study is exposure to VOCs in drinking water and specific birth defects and childhood outcomes. This was a multi-step process. It involved reviewing the scientific literature to identify which defects and childhood cancers were potentially associated with the contaminants and that we could possibly pursue.

Because at that time period that we're looking at there were no registries, we conducted a telephone survey to ascertain the potential cases. It was very important to us to verify the diagnoses because we were using self reports. We did want to obtain medical records to verify what was self reported. And then using that information we're in the process of conducting a case-control study.

So this slide shows the outcomes that

we chose to further study in the telephone survey. We were asking about neural tube defects, oral cleft defects, the following conotruncal heart defects, choanal atresia, childhood leukemia and non-Hodgkin's lymphoma.

So through the telephone survey to identify potential cases of those outcomes among the births occurring during 1968 to 1985 to mothers who resided on base at any time during their pregnancies, that would be they delivered on base or they delivered off base but the pregnancy was carried on base, we identified about -- we estimated, I'm sorry, about 16 to 17,000 births, and the parents of 12,598 eligible children were surveyed.

That's an overall participation rate of 74-to-80 percent depending on which range you use for the estimated births. Because there is not a really clear handle on the births that were delivered off base, we have some best guess from the Naval hospital. That's why it's an estimate of how many pregnancies there were on base.

So through our telephone survey we were able to capture a sufficient number of

neural tube defects, oral clefts and childhood cancers to proceed further with the study of those outcomes. There were 106 reported cases broken down as 35 neural tube defects, 42 oral cleft defects and 29 childhood hematopoietic cancers. And as I mentioned before, it's very important for us to verify, get medical confirmation of those cases. And that process has been completed.

So the way that shaped up was 52 confirmed cases out of the 106 we were able to get medical records confirmation for 52 of them, and 51 of those parents were interviewed. That's 15 neural tube defects, 24 oral clefts, and 13 hematopoietic cancers. Thirty-two of those 106 were confirmed not to have the reported condition. Eight refused to participate. We could not get, one way or the other, whether they have ^ [the reported condition -ed.] or not, they refused. Seven could not be verified or there was no medical record.

And believe me we tried. We took extensive measures. For those cases that were reported to have an oral cleft or a neural

tube defect we offered them a visit with a doctor today for an oral cleft dentist so they could say with their evidence of an oral cleft if there was no medical record for the time or the same thing for the neural tube defect. But still, unfortunately, seven cases could not be verified one way or the other, and seven were determined to be ineligible. That could be because it turns out that the pregnancy did not actually occur on base or they were born outside of the timeframe and things like that.

So, as I mentioned, we conducted parental interviews and also included interviews of 548 controls. These interviews were conducted in the spring of 2005, and we wanted to get information on the maternal water consumption habits, the residential history on the base and up through the first year of life, maternal exposures during pregnancy and other parental risk factors.

And we conducted an extensive review of the base family housing records to verify the dates and location of where the mother was reported to have lived on base. We also used

birth certificates and other information that's available to try to determine where exactly the mother was on base.

And Frank's going to discuss the data analysis.

USE OF WATER-MODELING RESULTS IN THE EPIDEMIOLOGICAL STUDY

DR. BOVE: I'm going to present what we propose for the data analysis. First of all, we're going to do separate analyses of each of these birth defects and so we'll focus on neural tube defects separately, oral clefts separately, and then we'll split it up between cleft lip and cleft palate and then look at childhood leukemia and non-Hodgkin's lymphoma together because of the small numbers of non-Hodgkin's lymphoma.

It may be difficult to also split cleft lip and cleft palate because there are 11 cleft palates roughly, and I think there's 13 or so cleft lips. So we're talking about small numbers throughout. So this is going to be the difficulty of this study because these are rare events, and doing a survey, phone survey, is not the best way to ascertain birth

defects or childhood cancer, but it was the only way to do it at Camp Lejeune.

So next we'll evaluate the ~~contaminate~~ [contaminant -ed.] levels both as continuous variables and as categorical variables. We'll attempt to use smoothing methods to give us cut points for the categorical variables; however, again, because of the small numbers of cases, we may end up with ^, no medium and high for the categorical variable cut points.

Each contaminant will be analyzed separately. That assumes that there's one contaminant that's causing the problem, not a mixture that's causing the problem, and then we'll look at joint effects of mixtures.

So for neural tube defects first we'll focus on the confirmed cases and look at average and maximum contaminant level over the first trimester, over the period three months prior to conception to conception -- so that period as well -- and the average level in the first month of pregnancy since that's when the neural tube is closing.

For clefts we'll again be looking at average and maximum contaminant level in the

first trimester. Again, looking at the period three months prior to conception to conception. Again, some of these are difficult to precisely or accurately define because we know when the birth occurs. We have some idea what the gestational age is and so on.

And then we're going to look at the second month of pregnancy because that's when the cleft lip and cleft palate are forming and are vulnerable to exposures. Although it may shade into the early part of the third month, so we may combine the second and third month as well.

And then for childhood leukemia and non-Hodgkin's lymphoma we'll look at each trimester separately. Then we'll look at the entire pregnancy. That's not on the slide. We'll look at the entire pregnancy, look at the average and maximum of the entire pregnancy.

Then we'll look at the first year of child's life. We only got information of the first year of child's life on residents, so we don't have information beyond the first year

of the child's life although it may be possible to reconstruct that from housing records and not from the survey information if that is a recommendation. But we only have information on the first year of the child's life from the interviews of the cases of controls.

And we'll also look at, again, the three months prior to the date of conception to conception. Again, we're not sure when during pregnancy before the first year of life when the child is most vulnerable to these exposures that might cause leukemia or non-Hodgkin's lymphoma. And then finally, we'll look at the cumulative exposure over the pregnancy and first year of the child's life.

I thought you might like to see some real data. This is, we don't have Hadnot Point data, but this is Tarawa Terrace, those exposed who lived in the Tarawa Terrace housing areas. And you can see why we need monthly estimates because there is variability, quite a bit.

Some people move in and out. Sometimes the wells are shut, the main well at

Tarawa Terrace is shut down so that these months there's very little exposure to these months, very high exposure and so on. So I want to reemphasize why we need monthly exposure levels.

Now, we're planning two future studies, one on mortality, one a health survey. And for that monthly levels of exposure contaminant levels aren't as important as for this study. And we can talk about this future ~~studies~~ [study -ed.] if you want.

Data analysis, the typical way to analyze these data is using logistic regression. Again, I'll emphasize that the data is sparse for the cases so we may explore using conditional or exact methods. But again, with sparse data no matter what you do, you're limited by the sparseness of the data.

For confounders we'll use the ten percent rule including confounders in the model if they affect the ^dration by more than ten percent. And we're trying to keep the models as simple as possible given the sparse data. And then we'll explore the information

we got from the survey on water consumption.

Now, I've never found this information that useful especially when people have to remember many, many years in the past, but we'll look at it anyway and see if it sheds any light on the situation.

Last slide we're going to talk, we're going to conduct sensitivity analyses to look at exposure misclassification varying sensitivity and specificity of our classification of exposure to see how that might affect the results especially with sparse data. They probably were affected quite a bit so we have to examine that.

Additional analyses, we have some cases and controls with a very poor residential history. This is another problem with the survey, people trying to remember their residences 20-, 30-some years ago or whatever. They forget. They're inaccurate. We have housing records that help to confirm some of that, but some people may have crashed with other people.

There are all kinds of housing arrangements that may have occurred on base,

and so the housing records only go so far. They tell you where the sponsor lived, but not necessarily where the spouse and the rest of the family might have lived. And so we'll try to work with residential histories just to make sure all the cases that we interviewed and confirmed get into the analysis.

But we might also include some that haven't been confirmed yet and probably never will be confirmed because we just can't get the medical records for them. There's about seven of those pending that will never probably just determine whether they had the disease or not. We did an extensive effort to do that.

For clefts, for example, we actually paid for people to go to the dentist to get a confirmation that they had surgery for a cleft. And we tried everything to get the records for anencephaly, which is difficult, and spina bifida and for childhood leukemia we, again, made a big effort to confirm them. But again, seven cases that were reported in the survey we couldn't confirm yet. So we may include them in a secondary analysis.

Finally, we don't base our interpretations on P values. That's my thinking. We use these kinds of criteria. We can have a discussion of that if you want, but that's how we analyzed it and interpreted it. So, any questions for Perri and myself?

MR. HARDING: Ben Harding. If we go back to the table of the real data example for Tarawa Terrace, I'm not an epidemiologist, and I'm afraid that this might cause you a headache. But a question I have is, how could you use a table like this instead of having, for example, for child number one, I guess that's minus three months.

DR. BOVE: Yes, minus three months from date of conception all the way to the third month of gestation.

MR. HARDING: If those cells were, instead of having a single number in there, had either a range or an empirical CDF of values that were generated by a more probabilistic analysis of an exposure, how would that, would that make your analysis impractical, impossible, what?

DR. BOVE: Yeah, the relative position of

each case and control wouldn't change with that so in one sense, no. The difference would be if we tried to make an inference as to at what level we see effect and what level we don't. And I think that this data is not good enough both on the water side or the epi side to make that assessment. Right now in this situation with environmental epidemiology and drinking water epidemiology, we still are not sure about the effects of these contaminants on these outcomes.

We have one New Jersey study looking at birth defects and we have a few studies looking at childhood leukemia like Woburn, for example, and then that New Jersey study that was looking at all ages but found an effect with childhood leukemia with TCE. So we're still in the early stages of trying to make the associations, not trying to define exactly what level of TCE or PCE we might see an effect.

So in other words, yes, we can plug almost anything in there, and it won't change the relative position of the cases and controls, and it will still be able to

determine whether relatively higher levels seems to be associated versus relatively lower levels. Does that answer?

MR. HARDING: Yeah, thanks.

DR. HILL: Two things. I'm kind of uncomfortable with having numbers like this reported with three significant digits.

DR. BOVE: Right, I'm sorry.

DR. HILL: So just a general comment there.

DR. BOVE: Actually -- Morris, correct me if I'm wrong -- but I think we have more than three significant digits in the table and on the website, don't we? Right. So I actually reduced the number of digits.

But, yeah, I mean, again, it doesn't affect the relative positions.

DR. HILL: Right, it just affects the appearance of ~~decision~~ [precision -ed.].

DR. BOVE: Well, for 118, what would you put, 120 or...

DR. HILL: I would tend to round.

DR. BOVE: Round? Okay.

DR. HILL: I would tend to round. Mostly, it's conveying to people the precision of the number to my mind.

Okay, and then I had a question earlier on when Perri was talking. I thought what I understood was that in your initial assessment, you didn't have the results of the groundwater model so you were using some other estimate of concentrations at the wells to get, and then you used the groundwater model to refine that? Is that --

MS. RUCKART: You're talking about the 1998 study?

DR. HILL: Yes.

MS. RUCKART: Well, that was actually just based on crude exposure, whether they lived in an exposed area or not so at that time it was believed that one area was unexposed, and we got some new information that that area was exposed. So it was just based on yes, no, you were in an exposed area or not to take into account the water modeling at all.

So now, first of all, we found out about this error and then we are going to have more specific information from the water modeling. So it seems like a good idea just to redo that analysis.

DR. BOVE: For example, I think that there

were 31 births we thought were exposed to trichloroethylene at Hadnot Point because that's the only area we thought. And that was because we thought that Holcomb Boulevard treatment plant was online before June '72. In fact, we thought it was online at the start of the study, which is '68. Of course, that wasn't the case.

So if you now understand that Hadnot Point served that housing up until June of '72, there's more than a thousand births and that changes things quite drastically for that study. And we didn't have this kind of data or the Hadnot Point data that we will have. So we want to go back and reanalyze it.

DR. HILL: And was the problem that you were using Holcomb Boulevard as your --

DR. BOVE: Unexposed group.

DR. HILL: -- as an unexposed group and now it's exposed. So, you could now -- I don't know if you can. I don't know how to do this exactly. But I assume you need to identify some other group as your unexposed group because you need a control group in your experiment?

DR. BOVE: No, the problem --

MS. RUCKART: Well, first of all, there's still going to be unexposed because people would have been exposed at different time periods, and there'll still be unexposed --

DR. BOVE: ^

MS. RUCKART: There are still unexposed. They'll just be less than there was like before there was 5,000 unexposed. There'll just be less, but there still will be unexposed from that study. But we don't have to collect any more data. We still have it.

DR. HILL: But the unexposed are amongst the housing units in the same area, but they're --

DR. BOVE: From '68 to '72, June '72, any part of the pregnancy that's within that area, all we have are people exposed to either Tarawa Terrace or Hadnot Point. Now, Hadnot Point, so for that period of time will have different levels of contamination but no births that are totally unexposed.

From '72 on Holcomb Boulevard is free of contamination except -- and we'll discuss this later -- for an interconnection that's used during the summer months. But we can

take that into account. We'll take that into account in the current study, too. So from '72 onwards we'll certainly have unexposed to work from.

It's the before '72 that will be a little bit difficult unless part of -- but still, part of the pregnancy may have been off base. These people move in and move out. For that study they had to be born on base, but they could have moved on base in the seventh month of pregnancy, eighth month of pregnancy, so they're unexposed before that. So there'll still be some unexposed people even for the '68 to '72 time period, just not as many as before. Follow me?

DR. HILL: Yeah.

DR. BOVE: Let me take each period, '68 to '72 you have two water supplies, Hadnot Point and Tarawa Terrace, right?

DR. HILL: I understand that.

DR. BOVE: We don't know what the Hadnot Point levels are from '68 to '72. An important well comes online, what, '71, right?

DR. HILL: But the exposures are just based on where the people had residence, right?

DR. BOVE: Right.

DR. HILL: But they live in this community. They don't stay home all the time.

DR. BOVE: That's right. That's right. So we're looking at, we're emphasizing residential exposures. We don't have much information. I mean, people may wander all over base, that's true. We don't have an outside comparison group, outside of Camp Lejeune.

DR. HILL: And that's what I was curious about.

DR. BOVE: We will. We will for the mortality study and the health survey that we're doing next. And the reason -- well, two reasons why we didn't do it before. We thought there was a clean, unexposed group. So that study, but we can't really redo that study other than take into account we could take into account secondary exposure on base and call the people who were completely unexposed, those people who don't live on base until they -- during the period when they don't live on base.

For the future studies we're including

a comparison population from Camp Pendleton. Now, Camp Pendleton is similar in many ways to Camp Lejeune and unsimilar in other ways, but they both have hazardous waste sites on base, and the main difference is they don't have contaminated drinking water, at least as far as we know at Camp Pendleton. So that will be an outside comparison group for the future studies.

DR. HILL: Thank you.

DR. ASCHENGRAU: I just wanted to ask some more questions about the residential history. So did the people have to remember like a street address? What did they have to remember?

MS. RUCKART: Well, for the current case-control study, we had some information from this previous 1998 study as well as the housing records. So we would like give them a trigger. According to our records you lived at whatever, and we would just say the housing area. You lived at Tarawa Terrace during this time. Is this correct? And then they could say yes or no. And then that usually did not cover the entire period that we're interested

in, three months prior to conception to first year of life. So then we would use that as our starting point and then ask them, well, what about before that. Where did you live, and then go back as far as we needed to and then up in time. And so, as Frank was saying, it's pretty hard to remember where you lived 20, 30, 40 years ago so then we did cross-reference that with the housing records, and then made adjustments. And then also with birth certificates or just any other information that we were able to process.

DR. ASCHENGRAU: So it's not like I lived at 371 --

MS. RUCKART: No, no, there's some --

DR. ASCHENGRAU: -- they don't have to remember that.

MS. RUCKART: No, the housing records would have information that was that specific, but we were just asking about the broad housing area. Our records show you lived at Tarawa Terrace or Hadnot Point or Hospital Point.

DR. ASCHENGRAU: So everyone living in that area gets assigned, or in a particular month, gets assigned the same value for their

exposure?

MS. RUCKART: Yeah, we're not getting it down to the street level or anything like that.

DR. BOVE: But we did get, I mean, during the survey we did get the street name and sometimes street number from people. And from that we realized that there was another part of Jacksonville, North Carolina, that was called Midway Park. Midway Park is a housing area at Camp Lejeune, but actually, there's a housing area outside the base that's also called Midway Park.

And we found out that some of the people we thought were eligible, were actually living at the wrong Midway Park. So the survey helped, and they weren't in the housing records. That's why that triggered it to some extent. I mean, we had no record of these people living on base. So that was helpful because the survey clarified that.

DR. ASCHENGRAU: And then the last menstrual period, is that from like the birth records to estimate the conception or do you use the birth date and gestation to estimate the

conception?

MS. RUCKART: We don't have information as part of the survey on ~~OMP~~ [LMP -ed.], or we don't have birth certificates for everybody. So that is why it's kind of, we don't exactly know the three months before. That's why we have those several different time periods we're going to look at, you know, minus three, date of conception to date of ~~conception~~ [conception -ed.], and it's not exact. We really just have when they're born.

DR. ASCHENGRAU: So you're estimating it when they're born, and then you're subtracting --

MS. RUCKART: Yeah, we can't figure it out gestationally or ^ [date of last menstrual period -ed.].

DR. GRAYMAN: Walter Grayman. Just to clarify, you seem to indicate that you weren't looking at the addresses within the areas. Is that correct?

MS. RUCKART: Yes, when we assign the exposure, we're just going to do it on the broad level, Tarawa Terrace, Hadnot Point, the various places they lived on base. However,

as Frank was saying, as part of the survey they could report a specific address and then we can cross-reference that street to get the housing area. But we're not expecting people to be able to tell us the exact street. They could just say, oh, yeah, I lived in Midway Park or I lived in Knox Trailer Park.

DR. GRAYMAN: My concern really comes when you go onto the Holcomb Boulevard where we probably are talking about variation in terms of the concentration of the contaminants within Holcomb Boulevard which is different from the other two areas.

MS. RUCKART: Yeah, there is still different complexes or different housing areas within Holcomb Boulevard like Berkeley Manor or something like that. So we're not asking them were you served by Holcomb Boulevard. We'll be asking them for the specific, did you live in Berkeley Manor. Did you live in Hospital Point? Did you live in, you know, other areas served by Holcomb Boulevard.

DR. GRAYMAN: Thank you.

DR. BOVE: Yeah, we can distinguish the different housing.

DR. GRAYMAN: One other quick question on that. You brought up other activities besides residence. Did you look into work activities or is this not a very big issue back at that time?

MS. RUCKART: We did ask about that and can factor it in if we have enough information. And as Frank was mentioning, you know, the ten percent rule for affecting the model under estimate.

DR. BOVE: But very, very, very few of cases work controls had a job that involved solvents.

DR. BAIR: I guess my question follows with --

DR. HILL: What's the ten percent rule?

MS. RUCKART: Well, it's just kind of a rule of thumb, I guess, that epidemiologists use. So you have your crude model which would just be your outcome and your exposure. And you get a, let's say it just gives an odds ratio or a risk ratio. And let's say you get 1.5 just crudely looking at exposure and your outcome. Are these associated?

Then as you start adding in some other

variables like did you work with solvents or something like that, then if you add that variable also in with your exposures, you just would have let's say in this case three variables: your outcome, your exposure and your potential confounder, did you work with this chemical.

And if you just run that model, and you were to get an estimate that differed from, in my example 1.5, of more than ten percent, you would include it. But if not, you'd say, well, it's not really impacting our measure here so we're not going to add that. Because when you start getting too many variables it can make your model not run if you have sparse data. It doesn't really help you.

DR. BOVE: But some people use P values to determine whether you include a variable or not, and that would be really problematic in this study with low statistical power. So we try to make sure we capture as much of the confounding bias that we can given that there is also mis-measurement out of these factors as well most likely because of recall

problems. But still we would have a better chance of including the confounder in the model that uses ten percent than if we use P values or some other rule.

DR. BAIR: I guess the question I have follows on one of Walter's earlier ones. Was there any assessment of exposure at mess halls or at daycare centers? Were all the residents cooking in their own residence or were there communal meals at some locations?

MS. RUCKART: All these things you mentioned could affect exposures, but we just don't have information on that. I guess we're going to assume like non-differential --

DR. BAIR: Well, did the mess halls have different water supplies than some of the residences?

DR. BOVE: Okay, the mess halls, we're talking now about the barracks then if you're talking about the mess halls, and you're talking about -- correct me if I'm wrong -- and so you're talking about bachelors' quarters, not family housing.

DR. BAIR: So families all ate in the individual residences because knowing my

mother that would not be the case.

DR. BOVE: I can't say that they didn't go out and get a McDonald's or something during -- I don't think McDonald's was around back then -- but we're assuming that the major part of their exposure is in the home from consuming the drinking water and showering, which gives you an important exposure and a dermal exposure. So we're going to assume that.

I mean, there's not that much variability. We've looked at the data for showering and consumption of water. There really isn't much variability and they can't remember anyway, but I think that we're in good shape doing it this way. This is what we'd normally do in these studies. We really can't, I mean, you'd have to have a diary in order to determine all those different ways of exposure, and we just didn't do that.

DR. WARTENBERG: I assume you also do some sensitivity analyses so that if there, if there was an exposure estimates, you'll see what the impact would be on the --

DR. BOVE: That's right, we talked, yeah,

yeah.

DR. CLARK: Any more questions from the panel?

(no response)

DR. CLARK: Any questions from the audience?

(no response)

DR. CLARK: Morris, do you want to go ahead with the program?

SUMMARY OF WATER-MODELING ACTIVITIES

MR. MASLIA: Our schedule, which is good, which will leave lots of room for discussion and questions. And just back to a couple of housekeeping notes. I assume all the panel members see the booklet of slides that we prepared. I forgot to mention that. We do have some extra ones if people in the audience want to peruse them. We've got them in the cart here.

We also have the notebook that we gave out to the panel members if anyone in the audience would like to just peruse a copy. We do ask that you return it and keep it here because it is draft material, but Barbara may pass out a couple of copies if the audience would like to see it.

What I'm going to do is just give a general overview of the entire water modeling activities. I'm going to start very briefly on what we've done with Tarawa Terrace just so we're all on the same page for those who, panel members and members of the audience, who have not been with us since then. And then go into Hadnot Point very briefly. We have subsequent presentations and staff that will actually present very detailed information on Hadnot Point.

Throughout the water modeling activities, the epidemiological study came to us and gave us four goals and objectives to meet. And this is by order of preference, if you will. If all we could do was give them certain information, and at least wanted to know the dates of the contaminants that arrived at the wells.

If we were able to provide that information, then they would like to have the distribution of contaminants by housing location. That is, was it served by the Tarawa Terrace water treatment plant? Was it served by the Hadnot Point water treatment

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plant or the Holcomb Boulevard water treatment plant? Having that distribution they would like to have monthly mean concentrations, and I believe that's the numbers that Frank and Perri showed up on that table.

Is that correct, Frank? Those were the mean values. We obviously, if you see any of the reports we have ranges associated with those. I think Frank just showed mean values for an illustrated example.

And then, of course, we get into the subject of reliability, confidence, how confident are we, that is on the water modeling side, and the values that we are giving the epidemiologists. And just as an example, if you look at some of the supply well data from Tarawa Terrace of the wells, it may range from non-detect all the way up to 1500 parts micrograms per liter. And so the question is how reliable, when we give them a number, does it range that much or does it not range that much.

So getting back to this, and this will help, I think, clear up a little. We've got three housing areas, Tarawa Terrace and Knox

Trailer Park someone mentioned, served by both Camp Johnson and Tarawa Terrace. What's referred to as Holcomb Boulevard, and there's the Holcomb Boulevard water treatment plant, and the Hadnot Point area right here.

Initially, we assumed that Tarawa Terrace was completely exposed or continuously exposed I should say for the study period. And we assumed that the Hadnot Point area was continuously exposed for the study period. We also then assumed -- and I say we, that was the information that the epi study talked about, that Holcomb Boulevard was completely unexposed.

Based on some information and digging around, newspaper articles, some transfer of property documents that were provided by the Marine Corps, we estimated actually that Holcomb Boulevard really did not come online until June of 1972. Just for your edification that's based on one nice big picture in a newspaper showing a grand opening of the plant in August '72, and also U.S. government property transfer to the tune of $700,000 occurring in June of '72 which would be the

treatment plant, meaning it was completed and online.

So that's our best estimate as to when Holcomb Boulevard, so that's the difference in time from '68 to '72. Obviously, Hadnot Point did supply contaminated water or water with varying concentrations of contaminants to Holcomb Boulevard.

DR. GRAYMAN: Morris, what is French's Creek? Why is that designated differently?

MR. MASLIA: It's just an area that's referred to at Camp Lejeune as French's Creek. It's on the same water distribution system.

DR. GRAYMAN: As Hadnot Point?

MR. MASLIA: Hadnot Point, but it's referred to as French's Creek, and we just, but it's the same distribution system.

We also have, and we met this past November, I believe, with former and current operators. You have a question?

MR. PARTAIN: Just [to -ed.] elaborate on Dr. Bair's question about the housing. My parents -- I'm one of the [Lejeune babies -ed]. I was born in January of '68. My parents lived in Tarawa Terrace, and the

housing units there are self contained. It's like a neighborhood. You've got your kitchen, everything you need is there. The base is a self-contained unit.

My mother is French-Canadian, and at the time English was her second language. She didn't leave the base. Everything she needed was on the base, PX. The PX was located at Hadnot Point, the main side. All of her OB/GYN appointments were on the main side at the Naval hospital. The O Club, where my parents would go for their recreation, was on main side.

So we were exposed to both Tarawa Terrace water, which provided our family housing, and also Hadnot Point water, which provided the water for the O Club, the Naval hospital where I was born, and any activities they did on there. So these houses are just like you would go drive through a subdivision. It's not like a barrack or anything like that but family housing. Of course, when you're dealing with barracks, it's a totally different issue. I hope I clarified your question there.

DR. BAIR: Thank you.

MR. MASLIA: There's an interconnection valve here and a booster pump right here. And when Frank mentioned previously about intermittent mixing or interconnection, we had a meeting with former and current operators, ATSDR did, I think last November, and we also have some logbooks that have some entries into them.

And what it turns out as a general rule of thumb is that during the spring, which is dry in April, May, June, everybody's filling up the kiddy pools, sprinkling a golf course up here, and someone, they may need some additional water at Holcomb Boulevard. So they would turn on a 700-gallon-per-minute pump. At some point they switched that out to a 300-gallon-per-minute pump, and there's entries into the logbooks when they did that.

At the same time if this did not provide sufficient water, then they could go and open up this interconnection, which is referred to as the Wallace Creek valve, and water would flow that way as well into that site. So that's how you would get mixing of

water, contaminated water, even after '72 in this area during April, May or June in that time period. And Jason Sautner will speak more about this on the second day about that.

And so that's a big difference than Tarawa Terrace for the question that we have posed because at Tarawa Terrace the last panel recommended -- and rightfully so because we didn't the testing because all the supply wells fed into a central water treatment plant, we could use a simple mixing model and mix, and assume, which we did, that the finished water concentration at the treatment plant was the same water that residents received from the treatment plant. So that's what's different about this situation.

MR. HARDING: Morris?

MR. MASLIA: Yes.

MR. HARDING: Ben Harding. If you go back to that slide, it doesn't make complete sense that you'd be able to do both things in a water distribution system, open the valve and use the booster pump. The use of the booster pump implies that the Holcomb Boulevard system was running at a higher grade level than the

Hadnot Point. And if you open the valve, if that were the case, then you'd expect water just to flow back into Hadnot Point. So I just want to put that question on the table, and maybe Jason or somebody later can address that.

MR. MASLIA: There's also ~~Joe~~ [Joel -ed.] Hartsoe here who probably has more expertise since he operated the system there that could answer us. Our understanding was -- and, Joe, please correct me. As I stated if there was insufficient supply from the booster pump, they would turn on, open up the valve.

MR. HARTSOE: The valve you're talking about ^ [is Marston Pavilion. -ed.] I don't ever remember opening that valve because of the watering of the golf course. It was always the booster pump. Then interconnections would only be opened if, that interconnection would only be opened if there was a major water break or anything like that. I don't ever remember opening that valve just to furnish water for the golf course area.

MR. MASLIA: There's also a two-week period in January of '85 when there was a fuel line

break at the water treatment plant here, and BTEX compounds got into the supply here. So then they used the Hadnot Point water supply for about a two-week period. And there's actually some fairly detailed measurement, concentration data throughout the distribution system that we have. That's the other point to remember. Did that answer the question?

MR. HARDING: Yeah, it sounds like that valve was only opened under very rare circumstances.

MR. MASLIA: It is noted in the logbooks that we have when it, at least on there is notation that they opened up the valve, the Wallace Creek valve.

DR. HILL: So are you saying that the records you're seeing contradict what was said?

MR. MASLIA: No, not at all. I'm just saying when we have information or data, we prefer to refer to the logbooks. The logbooks specifically provide an incident that the Wallace Creek valve was open.

DR. HILL: And as far as you know, is that because some major main break or you just

don't know?

MR. MASLIA: Oh, we don't know. It does not necessarily give those other details. We've actually transcribed the logbooks. Actually, the logbooks are on the DVDs for Chapter A, that three DVD set. They actually, if you're interested, we can point you to which files so you don't have to look through 20 gigabytes to find it.

But that's what we have gone through those, and that's one of the purposes when we had the meeting with the former operators so we could understand clearly because we did see entries mentioning a booster pump. We saw another entry mentioning a valve. And for awhile there we were not quite clear on the understanding of that. So I believe we're on the same page now, and we understand the operations we have seen.

DR. GRAYMAN: It would be interesting to maybe have a chart which would show on a month-by-month basis the number of hours that the booster pump was on and the number of hours that the valve was open on Wallace Creek.

MR. MASLIA: Jason does in his presentation tomorrow have a chart showing from the pump side the hours and so on, and he will present that.

DR. HILL: So there was this period of time where along Holcomb Boulevard there was this spill, and so they shut that water off. They brought water in from Holcomb Point, and during that time they did detailed monitoring of the quality of the water being delivered?

MR. MASLIA: Yes. I believe the state came in also and took some samples.

Is that right, Scott?

Yes, the State of North Carolina came in and there's actually sampling throughout the distribution system.

DR. HILL: I hadn't heard of that occurring, and it seems like that's a really nice opportunity.

MR. FAYE: That's discussed in detail in your three-ring binder report there. I think it's actually Table 12 or 13 of the Contaminant Data Report shows the analyses, the time of analyses, the location of the analyses. And there was the actual what we

would call detailed sampling only occurred for probably a couple days, but then there was periodic sampling at a smaller number of locations for actually about two weeks.

And all of the data that we have regarding that incident and the sampling and et cetera, is on, like I said, Table 12 or Table 13, and actually may not have been printed out, but it's on the CD that was provided with the binder.

MR. MASLIA: I can pull that up. If you'd like me to pull that up right now, I can.

DR. HILL: Oh, no. I would suggest going on with your presentation. I went through most of those tables and marked them so let me look at those, but I didn't understand the significance of them.

DR. KONIKOW: Just one question on those ~~detail~~ [detailed -ed.] datasets. Could that provide an opportunity to test or calibrate your water distribution model?

MR. MASLIA: Absolutely.

DR. KONIKOW: Okay, absolutely.

MR. MASLIA: Yes, that's at least one thought that we have, but that kind of data we

don't have otherwise. So, yes, Lenny, that's the lines, at least right now, that we're thinking along.

MR. PARTAIN: One important thing to note, I don't know if you pulled that dataset for the North Carolina testing in January of '85.

MR. MASLIA: Let's see if I can.

MR. FAYE: If you go to my hard drive --

MR. MASLIA: What table was that, Bob?

MR. FAYE: There you go. Go down to the tables.

MR. MASLIA: What table?

MR. FAYE: I think it's 12 or 13.

DR. HILL: It's 13.

MR. MASLIA: You want Figure 13?

MR. PARTAIN: Okay, that's it. Now, what I want to point out, these are different sample points along Holcomb Boulevard and Hadnot Point. The January leak that they're referring to that this dataset came from was the result, was taken after the Holcomb Boulevard plant had supposedly been cleaned because of a fuel spill.

Now, at this point in time, there was only one contaminated well operating that

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produced these results. The other ten, I believe it was ten contaminated wells had already been taken offline at the time of this reading. So you have one well producing those results all along different points of the distribution system within Holcomb Boulevard.

MR. FAYE: That's all discussed I think pretty thoroughly in the text of that report that discusses this incident and that was Well HP-651 that the gentleman was referring to.

DR. GRAYMAN: And that time period was when it was being supplied from Hadnot Point still?

MR. FAYE: Yes. And the issue there was that earlier during December of '84, I believe it was December 16th of '84, Camp Lejeune did a major effort of sampling all of their active supply wells because of their alert that they had, that there was several of the wells had been contaminated. And obviously, they were on a mission to find out which ones.

Unfortunately, part of that sampling effort, I believe, there were four of the bottles that were broken at the time. And one of those bottles was 651, so it was never recognized by anyone that that particular well

was contaminated until these data came along. And then that was the last contaminated well that they removed from service.

MR. MASLIA: Yes.

DR. ASCHENGRAU: We just noticed that one of the sampling sites was the Berkeley Manor School, and that the TCE concentration's very high there. So I'm just wondering is it possible that some of the children in the study went to school there? 1985.

MR. MASLIA: Frank says that's a future study. The study goes from '68 to '85.

MS. RUCKART: The children in our study report, they're carried in utero, so they would not be at school. I suppose if the mother was a teacher at the school.

DR. ASCHENGRAU: What year was it? Aren't you going back to '68?

MS. RUCKART: Well, if the births occurred during '68 to '85, it's possible that the children did attend the school, but that would not be included in our study because we're just looking at exposures up to the first year of life. We are doing some future studies, and that will include as part of our health

survey, dependents.

DR. ASCHENGRAU: Okay, but maybe we'll recommend that you go beyond the first year of life for the cancer outcomes.

MR. PARTAIN: You'll notice, too, that the hospital is in that dataset. I think it's 900 parts per billion or something like that.

MR. FAYE: And I think the relevance of this is that, as the gentleman pointed out, this was just one well that was pumping at the time. There were many other wells that were providing water to Hadnot Point by WTP at the time, and so the actual concentrations from 651 were substantially diluted, and you still got these concentrations.

And the point is -- I think I pointed that out as well in the text there of the report -- that you have as long as these contaminated wells were operated routinely, you obviously had contaminants routinely delivered to the WTP and this just happens to be the best example of that that we have.

DR. BOVE: One other point about this is that, yeah, the high reading at the school, but this was a two-week period. The school

was free of contamination most of the rest of the time. But there are schools in Tarawa Terrace, and they got contaminated water as well so the child would have residential and school exposure. And we're going to be trying to capture this in the health survey, the diseases that developed after as they got older.

DR. HILL: But the school would also have been contaminated perhaps during those April through June time periods?

DR. BOVE: Right, we don't know. It depends on, yeah, this is Berkeley, yeah. We're not sure yet what parts of Holcomb Boulevard housing got the full brunt of that when they turned on the valve, and what parts didn't get the full brunt if they're going to be diluted of course. So these are questions we'll have to resolve.

MR. MASLIA: Scott.

MR. WILLIAMS: You may have to present to the panel that you have the well-cycling chart for that time period, so there's a lot of unknowns there. Morris has a well-cycling chart when all that sampling was going on, so

you can actually see exactly which wells were on what days. We don't have the resolution for ^(off microphone).

MR. FAYE: Morris, I think this highlights the, probably the principal challenge from the ground up on this is to understand this may affect the groundwater as well, how these wells were operated. This is the same thing with Tarawa Terrace. This is a huge challenge in reconstructing that, and I think we ought to spend some time talking about how that was done for Tarawa Terrace. How it might be done for Hadnot Point.

MR. MASLIA: And I've actually got some Tarawa Terrace slides so maybe I should proceed to those and maybe we can --

MR. FAYE: Can I address that, Morris?

MR. MASLIA: Yes.

MR. FAYE: First of all at Tarawa Terrace our main, we didn't have a lot of specialized data in terms of the operations of the wells at Tarawa Terrace. We do have those kind of data for this particular aspect of the study for this study, and I'll detail that in my talk. But the point to be made a Tarawa

Terrace was our main approach was to make sure that we removed an appropriate volume of water from the aquifer at a particular time and for a particular time.

And the well capacities were just used to distribute that volume of water. We can actually do various tests and Peter Pommerenk has come up with a, described a whole series of concerns and tests that he would recommend for this particular study. And we actually have the data that we can accomplish that, and I'll talk about that in my presentation specifically related to well operations.

MR. MASLIA: So for overview, again, wanted to just make sure we were all on the same page and understanding that exposure, exposed, non-exposed and the time frame of each in which you have the valve and booster pump.

I thought it would be interesting just to give a generalized timeline so, again, everybody understands the relationship of different, the study, different occurrences of treatment plants or supplies coming online. And, of course, here's our current health study going from '68 to '85. Hadnot Point was

the original water supply system on base. The base started around 1941, and it's presently still operating.

Tarawa Terrace based on information in the work details of the Tarawa Terrace reports, online from '52 to '87, and, of course, that was shut off after February of '87 due to contamination. And Holcomb Boulevard, as we said, came online in June of '72 and it's currently still operating.

It's interesting that the documented VOC contamination, that's where we have sampled data strictly from '82 through '87. That's all to our knowledge that exists in terms of specific contaminants such as TCE, PCE, degradation products. And so that is now, there's post-remediation or remediation data as they were doing RIFS reports.

But in terms of the water supply, that's what I'm referring to here, that's all we have. The historical reconstruction for Tarawa Terrace indicated that concentrations above the MCL, which is five parts per billion, for PCE in November of '57. And, of course, the water treatment plant was shut

down during February of '87.

And at Hadnot Point, which is why we're all here today, again, this is what this meeting is all about, but again, the contaminated wells were shut down by '87. So, obviously, sometimes in this time frame it became contaminated. Lenny?

DR. KONIKOW: With the documented VOC contamination, was that in all three, from all three water treatment plants and all three supply systems?

MR. MASLIA: In '82 they not necessarily went to the treatment plants, probably in late '84, early '85 is when they actually started going to the wells and the treatment plant getting half singles, if you will. There's actually some inferences because of THM readings being affected by VOCs or chlorinated solvents in '81 and '80, but that is from '85 forward that that's at the treatment plants. I don't believe we have any supply wells prior to '84.

Is that correct, Bob?

MR. FAYE: Well, the question was related first to the WTPs. There's two tables in the

report, I think six or seven or something like that, that actually show the, actually list the contaminant information that we have for both WTPs.

And I think to answer you question directly, Lenny, I'm not really positive there was VOC contamination noted through samplings at the Holcomb Boulevard plant during this time.

And, Morris, what was the question about the wells, the supply wells? What was that about?

MR. MASLIA: During this period, the sampling.

MR. FAYE: Yeah, that's all in the report as well. There's a large table in there showing the BTEX contamination and the PCE, TCE and derivative contamination at the supply wells and it covers this period. And I think that might be, I don't know. You'll have to look at the list of tables, somewhere between six and ten, something like that.

DR. HILL: The earliest year is '84.

MR. MASLIA: Yeah, the earliest year is '84.

MR. FAYE: For the supply wells, yeah,

absolutely, yeah. The earliest is July, actually of '84, July 7th of '84, I think is the earliest data that we have and then there's the '82 data relate to sampling locations within the Hadnot Point distribution system.

DR. KONIKOW: The Tarawa Terrace with the first arrival in November '57, if that was actually several years later, maybe even four or five years later, would that have any effect on the health study since the health study is '68 to '85? In other words would any inaccuracy in that first arrival --

MR. MASLIA: We actually did, Mustafa Aral did some well scheduling optimization and did different scenarios with different wells other than the ones that we calibrated for the model. And you could shift the time from '57 to '60, but during the course of the study it did not significantly affect at all the higher concentrations.

They all tended towards that level of that chart, the graph that shows in the finished water that all it shifted was, other than if you shut down, for example, TT-26. If

you shut down TT-26, both the data and the model would show that your finished water went down to practically no contamination at Tarawa Terrace. But if you shifted the cycling so that it didn't hit or arrive or pass the MCL, say, as you said, 59, 60, 61, whatever, did not significantly affect the higher concentrations in the finished water.

DR. DOUGHERTY: Just to continue on that, was there sensitivity to the contaminant mass loading date as opposed to the water production schedule?

MR. MASLIA: The actual date of the introduction of the contaminant to the system at Tarawa Terrace?

DR. DOUGHERTY: Yes.

MR. MASLIA: No, there was not. That was -- and I guess I'll refer to Bob, but that was derived based on the deposition of the owners as to when they began operating the dry cleaner.

But, Bob, if you want to follow up on that.

MR. FAYE: Yeah, there was a legal, a deposition obtained from the owners, the Metts

(ph), the Metts family I believe is the name that owned ABC Cleaners at the time. They described the onset of their operations. They indicated that they used PCE from the beginning of their operations and so we had a date, I think, of 1953 or '54, something like that, when the PCE was initially loaded to the subsurface as far as the modeling is concerned.

MR. MASLIA: We also had information just to bracket the actual value as to how much the Metts estimated they used during their process.

MR. FAYE: Yeah, they indicated that they continuously for the years of interest to this study anyway, continuously used between two and three 55-gallon drums of PCE every month.

DR. HILL: Mary Hill. So I understand how that the rest of the modeling concentrations would change as that beginning date changed, but in terms of the epidemiology study, and their efforts to try to get time connections, are their results impacted by that?

MR. MASLIA: No.

DR. HILL: I thought not. I just wanted to

verify that.

MR. MASLIA: No, they would not be.

MR. FAYE: There's another question.

DR. BAIR: Yeah, it might be more appropriate for later on, but in terms of amount of contaminants going to the water treatment plants coming from the wells. The wells are constructed in a manner that commingles water between different aquifers?

MR. FAYE: Correct.

DR. BAIR: And I'm wondering in the Tarawa Terrace as well as the future modeling being done at Hadnot, how the quantity coming from each aquifer is apportioned relative to the total pump from the well because that makes a huge difference as to what's going to go to the water treatment plant. I mean, if you brought up 651, which was the worst well, that's open to three aquifers and there are screen blanks across two confining beds. So in terms, let's say it pumped 100 gallons a minute just for sake of discussion, did 70 percent come from one zone based on its permeability and thickness and 20 percent from another and ten from another? Because that's

really going to impact what goes to the loading to the water treatment plant. So if that's in the mix, you know, I'll wait to hear it then.

MR. FAYE: Well, the concentration at the well is a concentration of the mass of the water and the mass of the contaminant from all of the contributing units. So it's a, we could break out the individual contributions from the individual aquifers, but I fail to see how useful that information that would be --

DR. BAIR: Well, you have to assign a pumping rate to each zone in the well, don't you?

MR. FAYE: ^ is the concentration ^(off microphone).

DR. BAIR: But in the flow model, the flow and transport model, if those are not apportioned properly, then you're going to get a different velocity distribution coming to one zone and another. And the velocity distribution affects the concentration.

MR. FAYE: Well, like I said, we could break out the individual contributions, but it's

entirely mixed compute with the end concentration that the well delivers to the WTP, so I fail to see, yeah, we can do it just for academic purposes.

DR. BAIR: No, this is not an academic.

DR. KONIKOW: This is, you're using the models to compute the concentration coming out of the wells, and how you treat the wells in the model makes a difference is what Scott's saying. So the question is, how did you represent the pumpage in the model? Did you use the well package of ~~mod flow~~ [MODFLOW – ed.]?

MR. FAYE: I see.

DR. KONIKOW: In other words you have data that you used to estimate the monthly pumpage --

MR. FAYE: Right.

DR. KONIKOW: -- from each well. Some of that comes from the shallow system. Some comes from the deeper system. The concentration of those two units are not the same.

MR. FAYE: Where the well was in two aquifers in Tarawa Terrace which was basically

what we had to deal with there was just two aquifers, I'm trying to recall. I think for the most part I just subdivided the assigned pumpage equally. I had no basis for doing it any differently.

DR. KONIKOW: What are you going to do in the new models for Holcomb Boulevard and Hadnot Point?

MR. FAYE: We would have to look at it in terms of the, like the Trans-Pacific [transmissivity -ed.]and American [word incorrect, correct word unknown -ed.] are different units, and try to apportion it as appropriately as we can. I, frankly, haven't thought about it a whole lot.

DR. KONIKOW: Because this, as Scott says and I agree with Scott, this could make a big difference in how you, how much pumpage you --

MR. FAYE: I agree if contaminant is isolated to one unit, and that unit is poorly pumped or vigorously pumped obviously, yeah, it's going to make a big difference. I agree.

DR. KONIKOW: Have you thought of using the multi-node well ~~passage~~ [package -ed.] because that will do a lot of that automatically for

you.

MR. FAYE: Yeah, we have thought of that, and I think that's registered somewhere in the text there.

DR. GOVINDARAJU: Well, I just wanted to follow up on that but some of this was brought up at the discussion. Eventually, whatever the model does, what is ^ established in the well. So in the well water when it comes in from whichever aquifer, it gets mixed up. So the measured concentration is always a particularly average value.

MR. MASLIA: But basically, we've hit on Tarawa Terrace back and forth, which is fine. I thought I would just get back to the expert panel, the previous expert panel's, most people here were on there, and go over. There were five generalized recommendations. Some had sub-recommendations obviously for obtaining the groundwater modeling and sub-recommendations of doing sensitivity analyses, and dispersion fate and transport and so on.

But what I put together is just a table in Chapter A, which I believe was sent to you and it's on line and all that where we

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applied the recommendation and wrote the report in the manner so that anyone could pull, go to the expert panel report and see what the recommendation was and find a section in the report. If anyone wants a hard copy of this table, I could make that available.

But that's basically the approach, and hopefully, the approach coming out of this meeting is we'll have similar recommendations. When I say similar, probably more, but of that type that we can go down, and then the agency will implement as needed appropriately.

I thought I'd summarize the Tarawa Terrace -- and feel free to ask more detailed questions -- but in three major categories that the Agency feels that we achieved. And one was the understanding that the calibrated models for Tarawa Terrace are useful for the epidemiological study. Second, the concentrations that were measured in the 1980s, represent the high concentrations. There are no higher concentrations based on data and that was experienced over many years.

And finally, that using the models we would not be able to conclude when the

contaminated water reached certain values, such as arriving at the MCL, arriving at the water treatment plant and water concentrations people were exposed to on a monthly basis for use with the epidemiological study.

DR. HILL: I agree with this, but one thing I've thought about is the fact that the concentrations are not higher in previous years. Isn't that partly because of how the source is represented in the model? And are there situations such as high recharge events or something, was it ever investigated as to whether there might be circumstances that weren't represented explicitly in the model because it's an averaged, kind of a long-term thing but that might be more smaller scale events that could increase concentration?

MR. MASLIA: We did assume for the deterministic approach that we had a concentration. I believe it was 1,200 --

MR. FAYE: Mass loading ranges.

MR. MASLIA: -- mass loading ranges --

MR. FAYE: -- concentration varied over time.

MR. MASLIA: -- yeah, mass loading range was

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1,200 --

MR. FAYE: But to address Mary's question I think, yeah, they have ^ [massive -ed.] hurricanes there so you would get a dilution for a short period of time, but on the flip side, you get droughts that would increase concentrations for a relatively short period of time. So I don't know that we ever tried to address those kinds of cause and effect relationships in any of our modeling.

DR. HILL: And the one I was thinking of was that hurricanes might produce greater transfer of contaminants from the unsaturated zone into the saturated zone and which might show a ^ [relationship -ed.] of such.

MR. MASLIA: We did not address events such as those.

MR. FAYE: There was no continuous data to see if there were pulses or anything like that. We just didn't have that.

DR. HILL: I understand.

DR. KONIKOW: Just to follow up on that. Those high, rare, let's say, uncommon high recharge events might not lead to dilution, might actually lead to peak concentrations

because it would have the opposite effect of what you would want. Because some of the contaminant is hung up as a separate phase in some of it, and so the faster it flowed through a water during high recharge events could dissolve a lot more, just bring a lot more solute.

Because one of the things that I noticed in the analysis of it is that the problem with mass loading rate is when you match that with the fluid recharge rate that you use, you wind up with source concentrations in the liquid phase that would be perhaps ten times above the solubility limit. So there's an inconsistency there the way the contaminant is loaded into the model at least by using the mass loading. Or maybe that's too much detail.

DR. CLARK: ~~Over here~~ [Dr. Bair. -ed.].

DR. BAIR: Yes, I was going to ask if in the future model you're going to put together that's transient, would there be spatial and temporal changes in recharge that can account for droughts and flood events and was that used in the Tarawa Terrace model, transient

recharge, accounting for droughts?

MR. FAYE: We varied recharge only on an annual basis. That was our estimate. But to determine -- and we couldn't compute monthly hydrologic budgets. We just did not have raw data or examine the transporation date or anything like that. But what we did do was, we computed what we call a quasi or a gross hydrologic budget on a monthly basis for the period of interest using the climatological data that we had.

For example, we had pan evaporation data. We had rainfall data. So to estimate a month, this was an experiment just to test the sensitivity of the model to recharge. So what we would do, we would subtract the evaporation from rainfall and the difference we would assign as effective recharge. If it was negative, we would say recharge was zero for that month. Then we ran the model for all 528 stress periods with an array like that.

And then we compared the end-of-year changes in water levels using that approach versus the approach that was used in the calibrated model. And we found, and we did

that in the western part of the domain where there was very little or no influence of pumping so it would be just a natural ~~relationships~~ [relationship -ed.]. And we found that there was very little difference in the year-to-year changes using one method versus the other. And that's described in Chapter C in detail, the whole approach.

DR. BAIR: Did you look at changes in velocities? Because there's a difference between focusing on water level changes during that and looking at velocities during that. And it's the velocities that are going to drive the contaminants whether they slow up during a drought, but during a drought you're probably pumping more water, groundwater or during a flood or hurricane event or a really wet year.

MR. FAYE: The pumping rates didn't change using the [recharge rates -ed.]~~,~~ from the calibrated model. Pumping rates didn't change using the quasi recharge rates, and we did look at velocities throughout the model. But basically that was just an effort to find out where we possibly were violating the ^

[Courant -ed.] condition, not for the possibility you were talking about.

DR. ROSS: I've got a quick question that has to do, I guess, with recharge as well. ABC Systems or ABC Cleaners discharged via septic system. This answer may be in the documentation, but was the base plumbed on a waste water treatment system or was there a septic system associated with each house at any period of time or how did they treat their waste water?

MR. FAYE: How did ABC specifically treat --

DR. ROSS: Not ABC, but the base.

MR. FAYE: Oh, the Tarawa Terrace. That was a sewerage system. Yes, septic tanks as an issue of recharge, I don't think that that was anything to deal with.

MR. MASLIA: We're about five minutes from a break. And as I told Bob, the reason the breaks are so ^ [critical -ed.] and they might want to have one is because of the video streaming. They have pre-programmed certain breaks in. So if we can go another few minutes and take a break and then just pick up, we can continue.

But while we're talking on it, this, of course, appeared in the Chapter A report. This is from the deterministic calibration that we did at TT-26, the primary. And as you see, as we have noted, when that shut down for maintenance here, of course, the finished water concentration, the water coming from the WTP, mixed with the WTP, also dropped.

And, of course, this was the probabilistic, we had two probabilistic analyses. The blue line here represents the calibrated finished water. This is just finished water concentration that I just showed you previously.

We ran one scenario where we used the calibrated pumping schedule that Bob talked about in the calibrated model unadjusted but then assigned probability distributions to all the other parameters as noted in the Chapter I, hydraulic conductivity and infiltration and there's contaminant parameters as well and that's the yellow band from here to here.

And then the pink band we tried to assign a statistical or an uncertainty property to the pumping so that it varied

continuously, and that's detailed in the Chapter I report, Uncertainty, and that's the band, the pink band.

And I suppose what we observed is that the data, the measured data that we have, which obviously is in the late '80s, did fall in the confidence bands and was in the, for the water treatment plant, was in the calibration target, so I'm sure we'll talk a lot about calibration targets. There've been some good discussions in the pre-meeting notes about that.

But what I'd like to do --

And, Barbara, if you can get, I think it's the third or fourth poster. What I did I took this to the water treatment plant for both scenarios. And rather than calibration targets, I plotted it in terms of the 95 percent of the Monte Carlo simulations. So that's your confidence, the pink line going down there.

That's all the data that we have. This is all the data that's above non-detect. All these are detect measurements below detection limit either indicated as non-

detects with no symbol or in this case for example, we've got a below detection limit with a value of I think about six micrograms per liter.

And here the actual measured data -- well, that's the 95 percent of the Monte Carlo simulation for those particular runs with scenario one where pumping was not varied from the calibrated and scenario two where pumping was varied from calibrated value assigned a statistical value properties.

MR. HARDING: Morris, if you could go back.

MR. MASLIA: Okay, let me back up here.

MR. HARDING: I just want to give you an impression. And my impression in looking at this was these seem too narrow. I would expect to see a lot more uncertainty. That's just, I want to give you my impression. I have some specific questions related to the sensitivity analyses, and they're things we can talk about later, but just...

DR. HILL: Mary Hill. They do look a little more reasonable on an ~~arid landscape~~ [arithmetic scale - ed.].

MR. HARDING: Yeah, but looking at just the

arrival times, for example, very narrow.

DR. KONIKOW: Well, I think these are confidence bands assessed with a given conceptual model, with a given numerical model to look at the effects of uncertainty in just a few selected parameters. I agree. They're way too narrow in terms of what real uncertainty is.

DR. CLARK: I'm going to use my prerogative here as Chairman to say that we're going to take a break.

(Whereupon, a break was taken between 10:15 a.m. and 10:30 a.m.)

MR. MASLIA: Y'all get an A-plus for using a microphone except the people in the audience, the court reporter cannot hear you sometimes. So wait until you get the mike in your hand before speaking.

Bob, are we ready to begin?

DR. CLARK: Let's roll.

MR. MASLIA: We'll pick up where we left off, and I think just two comments I got cleared up. I guess the first one is there appeared to be some confusion about the valve and the booster pump. Let me bring the slide

up. The booster pump is right here. That's the 700-gallon-per-minute or 300-gallon-per-minute pump that I said was noted in the logs. And it ran intermittently April, May or June. And Jason will also have some information on that when he makes his presentation from hourly information.

The shut-off valve, and I believe we refer so there's less confusion, as Marston Pavilion that's close to Wallace Creek 'cause this is all Wallace Creek. And that's where they had to actually go in by hand -- if you can travel the bridge here, you'll see it's down below -- and actually open it up by hand. So there are two different hydraulic devices so to speak. And that's where Joel said he did not remember opening it up once.

I think we've seen -- correct me -- once or twice in the logbooks, Jason, that they said they opened up the valve?

MR. SAUTNER: It really depends if you want to count the period in January to February of '85. It was open for a nine- or ten-day period there. Besides that it was opened maybe five times between 1978 and 1986.

MR. MASLIA: So just wanted to make sure we were all, understood that if there was any confusion. And then during the discussion as to apportioning over at Tarawa Terrace where wells may have been open to different zones at Tarawa Terrace as Bob Faye pointed out, were only open to two aquifers, and ~~tran-posivities~~ [transmissivities -ed.] were approximately the same for each. Obviously, that will be different for Hadnot Point. That will be taken into account. We do have the multi-node well package to use.

And then finally, Lenny, for my own edification, when we get here to make it clear that we did use the same conceptual model in running the two uncertainty analyses. In other words we did not change the conceptual model or change boundary conditions or anything of that nature or change how the contaminant source was applied to the model, a constant source versus a injection-type source. Just wanted to clarify, just make sure. I think that was Lenny's point.

HADNOT POINT/HOLCOMB BOULEVARD PRESENTATIONS AND PANEL DISCUSSION

So we will continue on over at Hadnot

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Point. I'm, again, very briefly just going to show where we currently are from a project standpoint, and then we have follow-up presentations and discussions.

We're basically 95 percent complete with data analyses, the data that we have. That was the data that was presented in the notebook.

We're not 95 percent complete?

MR. FAYE: Yeah, for the IRP sites.

MR. MASLIA: Good, that's what I'm reporting on.

MR. FAYE: Good, say the IRP sites.

MR. MASLIA: The IRP sites.

DR. GRAYMAN: What are IRP and what are UST?

MR. MASLIA: The UST are underground storage tanks.

DR. GRAYMAN: And the IRP?

MR. MASLIA: IRP are the --

MR. FAYE: Installation Restoration Program sites and that terminology may not be exactly correct. Perhaps the folks from Camp Lejeune or the Navy can clarify that. But just for our own purposes of organization, that's how we've subdivided up the general data that we

find.

MR. MASLIA: The data report, again, the draft is what we provided you. When I say 95 percent complete, it's not going through review or anything like that, but in terms of compiling the tables, things like that, state properties, statistical analyses 95 percent complete.

Groundwater flow and transport modeling, obviously, we have not gone very far on there for a number of reasons. One is we want feedback from this panel. We have to provide you with some guidance as to the direction we were heading, and we tried to do that, but not yet commit a whole lot of time and resource.

Number one, we needed the data analyses to be complete. And then also, again, obviously, we need input from this panel. And the water distribution system modeling, we do have calibrated all pipes modeled for both Hadnot Point and Holcomb Boulevard that is based on field work that we did in 2004.

We conducted some initial simulations,

what were referred to as interconnection scenarios. That's where we turned that booster pump on and off, the 700-gallon-per-minute, and Jason will report on that tomorrow and that.

As Bob indicated, this refers to the IRP sites. We have since March, we know we have at least 100 more reports containing some form of information, and we can discuss that. We have a session on the second day to deal specifically with the concept of, I guess, more information. You have an expanding universe or a universe with no bounds with information. Some of it's usable; some of it's not.

And the question is, is where do you put the bounds on that to complete, as Dr. Sinks said, to complete the study in some amount of time frame. Perhaps there's an opportunity to use the data from here, what data is there as a second set of data, calibrate or get some initial estimates from a model, and then test it against the second set of information.

This is an opportunity we did not have

at Tarawa Terrace, so that may lend itself to addressing some of the issues as far as testing the model against a second set of information. And we have allotted some time tomorrow, but we can obviously discuss it now.

DR. BAIR: Hi, Morris, with respect to the data you have here, this doesn't include the well packets. The three-ring notebook makes a point of showing, I think it's an example of Well 663, HP-663?

MR. MASLIA: No, I know what you're talking about. We received ten years of, the most recent ten years of, we refer to them as well packets. Those are handwritten notes that have been scanned in. And we are, this summer I've got a --

DR. BAIR: Intern.

MR. MASLIA: Yeah, with the last name of Maslia that's not busy for a month or two during the summer who will be putting them in into Excel. We've got the Excel templates set up and they go from '98 to 2008.

DR. BAIR: I mean, one of the things I was scrambling to find in all the information and on the CD was the depth of the well screens,

the length of the well screens, the pumping rates of the well. Is there a central database that has that in it? That shows what formation each screen is in? the diameter? the length?

MR. FAYE: Well, I guess you just didn't scramble enough because there's definitely a lengthy table in the, on the CD. I don't know whether it was printed out in hard copy or not, but was it Table 5 that gives a complete description of the well, the well construction, the contributing aquifers, land surface elevation, the names, the a/k/a names. I think it's a fairly complete listing of the supply wells, the irrigation wells at Camp Lejeune.

DR. BAIR: I found that. What I couldn't find to tie into that was the pumping rate of that well or the pump capacity.

MR. FAYE: That's the capacity history information and that is in a separate package. I'm not sure if that was on the CD or not. But all the well screens and the other parameters that you mentioned were in that table.

MR. MASLIA: We can provide, as a member of the expert panel, a draft copy of that for you if that assists you with doing that.

DR. BAIR: I mean, so one of the questions I have, and I guess I'm just lumping it under data analysis, is there was, taking HP-651 as an example, they in another part listed a sampling depth in that well as minus 98 feet, and then listed TCE concentrations of 3,200, 17,006, 18,009. Was that a packed off interval so it just measured the UCHRBU unit or was that a vertically integrated sample?

MR. FAYE: No, all the samples were vertically integrated. I'm not sure where you -- we'll have to talk about that. That minus 98, that intrigues me. I'm not sure where that came from.

DR. BAIR: It's the middle of the upper screen of the three screens so it gets back to my comments about this vertical mixing and assigning appropriate pumping rates to each one of those in the model, but we can come back.

MR. MASLIA: Dave.

DR. DOUGHERTY: The one thing that was

missing in the well construction table, which is C-3, are the details of it. Is it sand pack all the way up? Are there ~~detnite*~~ [bentonite -ed.] seals or a similar type of seals at certain depths? Or are these just conduits from shallow depth to the screens?

The other related thing was the cross-sections that were shown in the same Chapter C from the IRP investigations show much shallower depths than the screens. Are we going to see some information that shows additional geology for particularly the 651 area? That was the one that caught my attention.

MR. FAYE: Of the approximately 100 supply wells, I would say upwards of 90 percent of those we probably have the detailed construction information that you're talking about in terms of the gravel packing, the sand packing intervals, depth to ground, stub index, the whole thing.

We have that information. It was just a matter of, in terms of creating a table picking the, what I thought was the most salient information and including that. We

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can generate all of that information. That's not an issue at all. And if it turns out that that's critical, we can just add another table to include.

DR. DOUGHERTY: But the ~~ground~~ [grout -ed.] interval I think is a significant one because that [^ - ed.transmission zone, if you will, we don't know whether they're isolated by zones or if there's connectivity --

MR. FAYE: Almost all of those wells are constructed in terms of transecting the individual confined units. If they're deep enough, they're probably gravel packed across the confining unit. The confining unit is breached, and they're gravel packed across that or sand packed.

DR. DOUGHERTY: And the grouting was this ~~official~~ [surficial -ed.] --

MR. FAYE: Yes, this just on the supply wells, typical 30 feet, 50 feet, whatever.

DR. DOUGHERTY: So they are open, basically, gravel tubes all the way from 30 to 50 feet of depth down to the bottom of the hole?

MR. FAYE: That's right, and even at Tarawa Terrace, I think there were two wells, two of

the older wells, where the bore hole was actually drilled substantially deeper than the finished well. And they filled the bore hole with pea gravel, the uncompleted bore hole with pea gravel. So, yeah, there are those construction issues. Like I say, we can generate all that.

DR. DOUGHERTY: That's the one that's pertinent to this and needs to be there.

MR. MASLIA: That's not a problem. That's a good question.

I think I've just got one more slide. This is just to give you really a sense of the magnitude and I think complexity. When we compare it side-by-side to Tarawa Terrace in terms of data availability -- we'll get into the model. The model is 25 times bigger -- but it's on the order of a magnitude more in terms of amount of data.

And right here I think the interesting is we've had our discussion, and as Bob has pointed out, we actually have supply well tests for Hadnot Point. We had none for Tarawa Terrace. So that just lists to give you sort of an idea of the volume of

information that we've gone through thus far and gathered as well as some of the complicating issues up here with a model that large. Rene will be getting into that. And that's it.

The follow-up presentations, and actually I think we start with Bob, actually provide much more detail. If y'all want to proceed with that. I think we're just about right on schedule or I can answer some additional questions.

DR. CLARK: Morris, I have a question that has to do with the distribution system modeling the, you know, we discussed this issue of potential contamination of TTHM samples by VOCs. And it struck me that where you had that interconnection problem, where you actually had measured samples in the Holcomb Boulevard area from the Hadnot Point area, if you had comparable THM values, we could compare against those. Then you get a good comparison to see whether that relationship if valid or not.

MR. MASLIA: That's a good point. I mentioned that also if we could do that, then

we could go back to the Tarawa Terrace early times where we have no VOC readings but we've got the THMs. And we see the THMs dramatically rising for a couple of years and at least give some additional confidence about that bound.

DR. CLARK: It should be possible to do that.

MR. FAYE: That might be very useful in the early parts of the period when we began actually to obtain data in the early '80s, so that might be a surrogate for that period.

DR. CLARK: And you should see the THM levels then go back down again as they take those wells offline so it would give a pretty good, it might track. It might or might not, but it might track pretty well.

MR. FAYE: The good part about that is that those data are fairly numerous, and they do span 1980 to well into the upper '80s period in time.

DR. CLARK: Well, they probably started collecting, I assume, on the base maybe about 1976? That's when the break, I think the requirements went into effect.

DR. DOUGHERTY: Nineteen eighty.

DR. CLARK: Thank you, Dave.

MR. MASLIA: That's something I think we want to go back and do not only at TT but also for Hadnot Point where, again, actual measured samples that we see are --

DR. HILL: Can I ask you a question? Are there any records, what are the records on the population of the base over the, from the '40s? How variable is that?

MR. FAYE: Table 2. Table 2 in the report.

DR. HILL: I'm sorry?

MR. FAYE: Table 2, Table 3, Table 4, something like that in the report. It gives the --

DR. HILL: The electronic table?

MR. FAYE: Yeah.

DR. HILL: Not this one. This one's --

MR. FAYE: It's one of the early tables in the, in your report there. It was probably on the CD, but it --

DR. HILL: Table 2 is Average Annual Rate of Treated Potable Water --

DR. CLARK: That's a different chapter.

MR. FAYE: No, it's in the background

section. It's in the housing area where I discuss the population over, there's several intervals of time there that I discuss the population at the different base housing units.

DR. HILL: If you can't remember, we can't either.

MR. FAYE: It is the report that's in the three-ring binder. It's the Contaminant Data report.

DR. HILL: I was saying I was interested in dates, the table reference provides the resident population of the different housing areas, but I was interested in base population because some of the contaminant sources we're talking about, the activity level at those sources I would think would be proportional to base population. And in this site like the industrial area, for example, or some of the carpal areas in Tarawa Terrace, they are clean. But here there are different things that you would expect the activity level to be proportional, I would think, to base population. So just if that seems --

MR. MASLIA: Frank, was not the base

population the assumption for the epi study that was constant over most of the time?

DR. BOVE: For Tarawa Terrace we have housing records and we can make some estimates as to the population there based on that. Now, the units, we don't ^ [know the number of -ed.] people in those units. The same with Holcomb Boulevard. We know when the housing units are built, so we can do that. But the problem is main side ^ Hadnot Point. We have barracks, and we don't know how many people went in and out ^ barracks ^ [during -ed.] ~~Viet Nam~~ [Vietnam -ed.]. We do have ^ [information -ed.] from the '70s on based on computerized data, but before that we just don't know. And the barracks are --

DR. HILL: But you don't have sort of population values for --

DR. BOVE: The health assessment that we just went through has estimates of what the population ^ is today and the recent past. We don't know how many people went through those barracks during the ~~Viet Nam~~ [Vietnam -ed.] era and before.

We have computerized data -- and

Scott, correct me if I'm wrong -- We have computerized data from '71 on although from '71 to '75 we don't have their unit code so we're not sure who was at the base even then. From '75 onward we know how many people were at the base but we have family housing. So we have some information for -- we have Tarawa Terrace and Holcomb Boulevard were pretty, we can have good estimates. It's the barracks. It's the barracks that have trouble before '75.

MR. WILLIAMS: There are certain ways we can estimate it, but, no, we don't, we didn't do base ^ [census -ed.] or anything like that. There was a base master plan that came out like '87 that has 1983 data. Morris has all those where they actually did go to each water system to estimate how many people were served by that water system. It was very, they don't reveal the method they used, but you can tell by ^ [? -ed.]22,223 [? -ed.] people on this water system, and you can use that to estimate. You can say if there was this many people on these water systems and project that before '87 back to '57, you can get a crude

estimate of how many people were served. And then you can assume the military persons would have had a two-year residency on average. Sometimes it was higher than that; sometimes it was lower than that. You can really get a crude estimate of the population. And that's how we came up with approximately 500,000, and that's probably conservatively high.

DR. CLARK: Let's move on at this point. I've got two more questions and then I want to move on to Bob's presentation.

DR. KONIKOW: Morris, on your last slide, on the availability of data I have two comments and/or questions or one comment and one question. One is that you're showing there's a lot more data available for the Hadnot Point area.

MR. MASLIA: We've got a hundred ~~USD~~ [UST – ed.] reports.

DR. KONIKOW: Well, you show there's more wells, more water levels.

MR. MASLIA: Oh, yes, yes.

DR. KONIKOW: So in terms of the, let's say, practicality of doing the detailed, deterministic models, I wanted to point out

that if you look at the density of the data, it's actually much better in the Tarawa Terrace. It's about 105 wells per square mile in that area. Whereas, if you go to the Hadnot Point, it's only about 17 wells per square mile. So even though there's more data, it's more spread out, and that just makes it much more difficult to do the modeling and get the resolution that you need.

MR. MASLIA: Are you speaking from a deterministic standpoint?

DR. KONIKOW: From the deterministic groundwater model.

MR. MASLIA: Right, we'll address that. Rene will, but I would say probably 90-to-95 percent before we made up our minds to go with ^.

DR. KONIKOW: The other comment I have is that you're showing quite a few well tests, pump tests in the Hadnot Point area, and I'm assuming that these give estimates of transmissivity or something that correlates with transmissivity. And yet in the model, at least in the first steady state model, you're assuming each aquifer is homogeneous.

Can these data and all these tests be used to look at spatial variations in transmissvity and try to incorporate that information into the model to get better resolution and better matches on the head distributions?

MR. MASLIA: Yes.

MR. FAYE: Do you want me to answer that?

MR. MASLIA: Yes, go right ahead.

MR. FAYE: Yes, but the vast majority of those aquifer tests, Lenny, are for the Brewster Boulevard aquifer. So, yeah, which was obviously the, that's the aquifer that receives the contamination. So for that particular layer, probably for the layer representing layers, the layer representing the Tarawa Terrace aquifer, there may be enough data out there to provide some kind of gross detail resolution of the hydraulic characteristics.

DR. KONIKOW: Are you planning to do that?

MR. FAYE: Yeah.

DR. CLARK: One more question right here.

DR. ROSS: This relates to, I guess, variability in source streams. Perhaps it

also relates to population changes over time. I expect during the ramp up to the ~~Viet Nam~~ [Vietnam -ed.] War there'd be more Marines passing through the base; therefore, ABC Cleaners would be cranking through probably more than two or three drums of perc [perchlorothylene or PCE-ed.] per month. Was there any consideration about that?

MR. FAYE: That doesn't seem to be the case. I mean, that was specifically addressed in the interrogatories during the interviews of the family and the owners. They had hands-on. I mean, that was their business. And you have to remember, too, now that there was a laundry, a major laundry, at the base itself. So they were possibly or probably dividing up the available work between them. So, but Mr. Metts was very specific, and he was asked that question specifically, and it was two-to-three 55-gallon drums of perc every month.

DR. ROSS: Did the base want them to use perc and what did they do with that?

MR. FAYE: They used barsaf* [Varsol -ed.] up to the early 1970s and then they used perc. And we do not have any records of their rate

of use. At least we don't at the present time.

MR. PARTAIN: ^ [Where is the base laundry? -ed.] (off microphone).

MR. FAYE: Site 88, Building 25.

MR. PARTAIN: And there is a PCE ^ [plume -ed.] there.

MR. FAYE: Yeah, oh, big time plume.

DATA ANALYSES -- GROUNDWATER

DATA SUMMARY AND AVAILABILITY

My name's Robert Faye. I work for the Eastern Research Group and I support the Camp Lejeune Project here. For the Hadnot Point and vicinity project my basic responsibilities have been locating data, recognizing data that will be useful to the project, processing that data, creating databases, writing one of the reports that was in the three-ring binder there that you all received, The Soil and Groundwater Contamination Report. I apologize it wasn't completed, but it was 95 percent completed and there's only so many hours in a day.

This is a summary of available pumpage data that we have, daily operation schedules for Hadnot Point WTP individual supply wells.

We have daily operation schedules from November 28th, 1984, to February 4th, '85. Scott alluded to those data earlier when we were talking about the BTEX spill at Holcomb Boulevard.

As far as our corresponding pumping rates for both the Hadnot Point and the Holcomb Boulevard WTP individual supply wells, we have that data for a several month period here, from October of '88 to March of '89. Total gallons pumped, average pumping rate, average daily withdrawal and percent of time inactive for HP and HB WTP. The supply wells 1993, we have that data from that year. And as Morris was alluding to earlier, we have daily logs for wells pumped indicating operational status on and off for individual supply wells at both Hadnot Point and Holcomb Boulevard from January 1998 to June of 2000.

And these data to a large degree will allow us to address a number of the questions in terms of accommodating actual well operation scheduling in the HP/HB model that we're contemplating that you folks are commenting on here today. Peter Pommerenk in

his notes address those issues in good detail, and I think these data will allow us to accommodate a lot of that, a lot of his concerns.

These are data that we have relative to either supply of water, water delivered or both for the WTPs. The first two lines there, Annual Delivery Rates, those are tables in the three-ring binder and the Soil and Groundwater Contamination Report that I wrote in Tables 3 and 4. I can't remember the names now, but they're all listed in there. Delivery rates from Hadnot Point, '42 to '98; Holcomb Boulevard, '75 to '98.

And then we have monthly rates of well water supplied or and/or treated by the WTPs, September '55-January '57. January '80 to December of '84, we have some overlap here; January of '82 to December of '93; January of '87 -- and these data do not all agree for the same months so we have to reconcile that.

And then we actually have daily rates of well water supply treated by the WTPs for this period, January '95 to May '99; January 2000 to December 2005. So you can see we

have, at least as far as an annual situation, we're in pretty good shape. And through the whole period of interest that we would want to accommodate. And as far as the monthly rates not too bad either. And daily rates strictly for more modern times.

DR. KONIKOW: Bob, on the previous slide I'm still not sure. In your model you probably are going to go with a monthly stress period, right?

MR. FAYE: Yeah.

DR. KONIKOW: But with this kind of annual data how are you going to reconstruct monthly withdrawals from the wells to plug into the model?

MR. FAYE: Well, we actually have monthly rates of, we actually have several periods of time here, Lenny, where we have hours pumped, corresponding pumping rates --

DR. KONIKOW: That's all pretty recent. What about prior to 1984?

MR. FAYE: We'll probably use the same approach we did there in Tarawa Terrace where we apportioned a monthly rate according to the percentage of total well capacity. And that's

exactly what we did at Tarawa Terrace.

The objective there, as it should be here, is to remove a specified volume of water from the system. So in that case the actual capacity, the actual pumping rate becomes just a surrogate for apportioning based on a total percentage basis. But we can also, using these data, address a lot of the operational concerns and interests that several folks have addressed in your notes including Peter, who really got into it in detail.

We can actually run tests and change our stress periods to 12 hours and run for specified periods of time where we actually have data to allow us to do that, to tell us to do that, and check the differences in water levels over a month to see what those effects would be. And by extension also into the fate and transport models, see how it affects the simulated concentrations.

DR. GRAYMAN: But if you go to the next slide, I mean, it looks like there's that 23-year period where you have absolutely nothing finer than annual, and that's the major era, major period.

MR. FAYE: Yes, and that was similar to the same situation we had at Tarawa Terrace. We didn't really pick up on monthly WTP deliveries or supply water until 1975, I believe. So we went from '52, '53 to '75. And what we did, we took like a ten-year period where we had, where we actually had those data, took an average, and then assigned that as a monthly rate back in time. We considered that was the best average that we had.

DR. GRAYMAN: Was there, I mean, to go back to Mary's question if there was any kind of a population or census data at least you could use that as a surrogate for water --

MR. FAYE: Well, we did. We, in an anecdotal way we did because it was Tarawa Terrace. There was a finite number of houses, and we understood that that housing was full almost all the time. There was a demand for that housing almost all the time for our period of interest. And it was subdivided into two bedroom, four bedroom, whatever they were, and that was a consistent thing for the period of time.

DR. DOUGHERTY: So one way of apportioning the stress is based on their portion of the capacity, but is there a portion of the record that's sufficient where you could look at the behavior of the operators in terms of how they operated the system rather than how the well screens had the capacity and use that as a surrogate rather than --

MR. FAYE: Yes, as Peter pointed out most of these wells were probably operated, well, he says 12-to-16 hours a day, which is fine. We can simulate that kind of a condition, not for our whole 1942-to-2005 period of interest or anything like that. But once we have a model that we have confidence in in terms of close calibration, quasi calibration, however you want to term, however you want to categorize it, we can run then these tests.

We actually have data that can assist us in understanding how the system was working operationally for individual wells. We can run specific wells for specific periods of time based on the data that we do have. We can turn other wells on, turn other wells off, that kind of thing, and actually test on an

end-of-month basis how it affects, what differences there would be just using a monthly stress period or a 12-hour stress period, et cetera, et cetera. And that's fully reasonable, and we intend to do that.

DR. GRAYMAN: Bob, could you put up a figure if you have it, a figure of what the annual delivery rates were over those periods? Is there one?

MR. FAYE: I'm sorry, Walter, there is not, but there is in the -- I keep alluding to that report. There is a, there are two tables in that report, one for the Holcomb Boulevard plant and one for the Hadnot Point plant that shows the annual delivery rates for those periods that are up there.

DR. HILL: That's not one of the tables on the -- is it a table or a figure?

MR. FAYE: It's a table.

DR. HILL: And it's not the table on the --

MR. FAYE: It's like C-2 or C-3 or something like that.

MR. HARDING: They're Table C-2 and C-4.

MR. FAYE: Okay, there you go.

DR. HILL: A lot of years say N/A.

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MR. FAYE: No, that's not true. There's only a couple years that say N/A.

DR. HILL: In the C-2 there's one, two, three, four, five, six, seven, eight, nine, ten, 11, 12, 13. And then 69 and 70.

DR. DOUGHERTY: You can ^ [estimate -ed.] from the neighbors unless there was some significant population change, you can ^ [estimate -ed.] because it's ^ [stable -ed.]. In the study period it's the first, before the first five years.

MR. FAYE: Okay.

MR. HARDING: If you look, it's reasonably stable and reflects the change that was made in, what was it, 1972, when Holcomb Boulevard came on line.

MR. FAYE: That's right.

MR. HARDING: If you take that into account it's really fairly stable.

DR. BAIR: And I think the first two years of Holcomb Boulevard we don't have any of that.

MR. HARDING: Just as a placeholder because it's way more important -- well, maybe I shouldn't say that. I'll leave the

groundwater people to say how important the allocation of pumping to the different wells is. But I think when you start looking at the concentrations in the finished water, this becomes critically important on a fairly short time frame because we have a precision that's required here, the trimester, for some of this causation or whatever the epidemiologist calls this.

I'm trying to think of it. Association, there you go. And how the operators ran these wells is going to become really important. And so I'd like to have more discussion about that when we get to the water -- I think it's appropriate in the water distribution side of this discussion.

DR. BAIR: And that in turn is dependent on how the pumping rate is apportioned to each one of the lenses or layers that the well screens are across from, which in turn, is dependent on the confining beds in between them that are all given the same value of hydraulic conductivity ^ [in feet -ed.] per day.

MR. HARDING: Well, that will be physics

down in the well hole, and then above the well hole there's a guy that flips a switch that turns on a particular well. And the way they make that decision is what, once we've figured out the physics of what brings us to an average concentration at the well head, it's that flipping of the switch that's going to determine what the concentration is essentially for the most part that gets to people's homes, and that's the part I'm talking about.

DR. BAIR: It's defining the relative permeabilities in the sediments that determines which plume, whether it's at this level or this level or this level contributes what rate and what concentration to the well bore.

MR. HARDING: I understand, and the interface between the water distribution modeler and the groundwater modeler, we just refer to wellhead concentrations in the above ground part of it. So once you guys have figured out the wellhead concentrations which relates to all the physics that takes place in the bore hole, there's another question which

is when did the operator turn on the well and for how long? That's my issue.

MR. FAYE: Actually, it's even more complicated than that because there's --

DR. BAIR: Mary mentioned the three significant digits on that table earlier, too.

MR. FAYE: There's a routine operation that Peter constantly refers to, and correctly so. And then there's sort of an exceptional type of operation, and that's, and one example of that is this period of time in late January and early February of 1985 when a lot of the wells that were contaminated were taken off line. All of a sudden Holcomb Boulevard couldn't be used any more.

All of the water supply to that part of the base had to come from Hadnot Point, and they just turned those wells on and let them fly. So you have -- and so you have a situation where these wells were being pumped 24 hours a day, day after day. I don't know how frequently that kind of a situation occurred, probably not a lot.

But ancillary to that situation is for whatever reason most of these supply wells end

up on somewhat removed from the center of mass of the plumes that were recognized in the middle '80s, middle '90s, whatever at a lot of these sites. So what happens is if you turn the well on for 12 hours and sample it, you'll get one concentration of a contaminant. If you turn the well on for 24 hours for eight days and sample it, you've moved a lot more of that mass from the center, mass of contaminant from the center of the plume toward the well, and you'll get a much higher concentration.

And, indeed, we see that in the data, and that's exactly what happens. So there's a matter of routine operation, and then there's a matter of exceptional operation so that adds another level of complexity to the argument.

DR. POMMERENK: I want to chime in on this. Just like you said, it makes a big difference for the contaminant movement of whether you operate a well like for a month continuously at a reduced flow rate or whether you operate it at a designed rate for 12 hours a day.

MR. FAYE: Right.

DR. POMMERENK: I think that the uncertainty associated with this needs to be worked out

somehow and ^ [reflected in -ed.] the results.

MR. FAYE: Well, we have probably, what, two or three individual cases where we can actually test, use the model at some point when we have confidence in the calibration. At some point we can actually test that against actual field data for several wells which will give us some insight how the model's actually responding to that kind of condition. Right now that kind of a test and maybe some hypothetical tests would be perfectly feasible as far as I'm concerned.

DR. POMMERENK: I think at this point, I think in the near future you would have to develop at least some, a pilot study to just demonstrate what the potential uncertainties are, you know, operating in an idealized fashion versus what I perceive is more the realistic way of things, how things were done.

Another complicating factor is, of course, the fact that the total well capacity ^ [of the -ed.] well fields exceeded the required capacity for water demands that were at times 100 percent or even larger. So there were many more wells available than needed for

day-to-day average operation. In fact, the State of North Carolina currently requires your water demand can be met with 12 hours of pumping, and I don't know how far back this regulation goes.

But so the result of this is that the operator has twice as many wells available as actually needed. So given the right permutation for those times, we don't know which wells were actually being used to meet the demands introducing additional uncertainty. Because you could have, you know, on any random day or even if you go into further larger periods, a set of wells that were less contaminated than in other weeks a set of wells was used that were more contaminated. So I don't know how you're going to address this kind of uncertainty.

MR. FAYE: I think we can get a large handle, our arms around that issue, not perhaps easily, but I think we have the information to do that, Peter, right here with this set of data. We have actually daily operations on and off for dozens and dozens of the supply wells that were active at this time

during January '98 to ^ [2008 for -ed.] ten years. So there's a lot of statistical inferences in terms of operations. This 10,000 pages of data so that we can, there's a lot of statistical inferences that we can glean from that data.

And the good thing about this in addition, is that a lot of the wells that were active at this time replaced previously active wells going back 20, 25 years. So the inferences that we glean from this set of information, we can actually extend back in time to the early '70s, perhaps even late '60s and then maybe even beyond that if it turns out that there's some degree of consistency that we find to the way wells were operated back in the '50s or whatever with the other data that we have. So I think we can get our arms around that anyway from about 1970 up to the present time without a whole lot of trouble. I shouldn't say that. We can get our arms around that. It'll be a pain in the rear, but we'll get our arms around it.

DR. KONIKOW: Can you briefly describe how the well capacity data were derived? In other

words you, basically, you assumed that the pumping rate was the well capacity information. And what I remember from one of the tables is that for an individual well for month to month it looked like the indicated well capacity could vary 20, 25 percent.

MR. FAYE: Yeah, and particularly over time because these wells, well, some of these wells were used for three and four decades. Now they were periodically reconditioned and whatever, you know, pumps repaired, bearings replaced, et cetera, et cetera, of course. But you do have a deterioration in, expected deterioration in the well capacity over a period of time.

And we have a lot of data indicating what that is. What that deterioration was and then as some operational thing occurred, what pump replaced, whatever, and the capacity goes up. To answer your question more specifically with respect to the well capacity test, typically, what you and I would call these tests would be a crude step drawdown test.

And basically they vary the head that the well is pumping against by discharge and

check that pressure just to make sure that the well can meet its expected operational ranges. And that's essentially what they are.

They're step drawdown tests, and then typically, after the test there'll be a little note at the bottom of the test page that'll say left pressure at 100 psi or whatever it is. And that 100 psi then refers directly to a discharge that was used during the test, and that's the discharge that would show up in the Capacity Use Table that you're referring to at a particular, you know, October of 1978 or whatever it happened to be.

DR. DOUGHERTY: Just to go back to the operational uncertainty and how to reconstruct that, there's a marked change in data density in '98. And I assume a bunch of sensors went into the system. Was there a change in the operations going through a programmable controller or anything at that point which would suggest a difference in operation prior to those data?

MR. FAYE: I don't think so. They've been using a SCADA system over there for many years for better or for worse, but I don't know of

anything that demark -- delimited 1998 in particular as an effort.

DR. CLARK: We're going to have to move on. We've got a lot of other material to present, so...

MR. MASLIA: Bob, can I just answer that?

DR. CLARK: Okay.

MR. MASLIA: The reason there appears, I say there appears to be more data density is because after ten years or ten years worth of records, the records are destroyed. So in other words '98 to 2008 represents the most recent ten years of records that are kept.

MR. WILLIAMS: The State of North Carolina requires you to maintain ten years of the data, and so I don't know that they're necessarily destroyed. They're just not kept after, when it turns into the eleventh year. So that's why we have --

MR. FAYE: That's your answer. Is that good? Okay, let's go on.

This is the slide that Morris stole from me, and I'll try to make him regret that. He's wrong here in terms of the slide, and where supply well tests at Tarawa Terrace.

And, Lenny, most of these were just exactly what I was talking about. These represent those step drawdown efforts that were made during the capacity use tests.

Let's see, what else do we have? Well, this is just, as Morris pointed out, this points out the great difference in the numbers of data that are available in this study. And as we just briefly discussed earlier, this represents what we call IRP data. This slide sort of resembles a credit card application. There's little, fine print down here talking about these LUST reports that have just recently come to light.

Timing was good on that because we were just about finishing up the IRP data. We couldn't have dealt with any more data if we tried. But anyway these represent the numbers of data that we have for the Hadnot Point and Vicinity Study.

And, Lenny, I would quibble a little bit with your density numbers. What you should really do is pick out two or four square mile areas where we have data, where the data actually occur at Hadnot Point, and

you'll see tremendous differences in density in the areas that count. And I'll talk about that in a minute relative to Tarawa Terrace.

DR. BAIR: Bob, could you keep that on for just a second?

MR. FAYE: Sure.

DR. BAIR: Thank you. You mentioned that most of the 69 supply wells and 132 pump and aquifer tests are really these step drawdown-type tests?

MR. FAYE: No, not for these.

DR. BAIR: Not for the 132?

MR. FAYE: No, those probably represent completion tests by [the driller -ed.]^. It would still be, to a large degree they would still be step drawdown tests, but they would be a lot more detailed than a capacity use test.

DR. BAIR: So my question is are there or how many tests are there that are a bona fide aquifer test where you have an observation well, and we can extract from it a horizontal hydraulic conductivity from a specific zone, a ratio, perhaps an antisotropy within that zone so that it gives you some guidance for what to

use as hydraulic conductivity distributions at each one of the layers. And where did you get values for the confining beds? Are those part of that set, too?

MR. FAYE: No, no, these would all be the permeable units. These would all be what we would call the aquifer layers in the model, virtually no data. We have a little bit of data at one site at Tarawa Terrace that we could refer possibly to, a confining unit, and I think that was like a half a foot per day or something like that horizontal.

But let me see. As far as the supply wells, you can forget antisotropy. Maybe ten percent of those had a single observation well so you can compute storativity from that, maybe ten percent of those. Now, the monitor well tests are a lot different. There are multiple, multiple observation wells for the most part, but the pumping rates are so low because it's contaminated water, and they're trying to deal with it as a disposal issue.

So the pumping rates are so low that the best information you can get from most of the monitor well data would be like a distance

drawdown [curve -ed.]^. You don't get a lot of intervening time result at the observation wells.

Now, to flip that around there's probably several sites, I would say two or three where I was actually able to apply ~~a~~ ^[aquifer-test ed.] analyses, and actually compute a leakage for the intervening confining units. Also, there's quite a bit, in the supply wells there's a fair number of analyses that would lend themselves to like a Cooper-Jacob analyses, so it wouldn't be strictly a step drawdown.

DR. BAIR: Are those values, the variants there, put into the steady state model? Or is it still kind of a layered system with uniform values going across all the layers?

MR. FAYE: I didn't construct, I wasn't directly involved in the steady state model. Rene is going to address that. But I do believe that he interpolated the point data to the layer for that domain. The confining units are a whole 'nother story. They're sort of an arbitrary assignment right now. And one-tenth of the standard kind of heuristic

type approach and one-tenth of the permeable unit value. But I think that'll be refined later on.

DR. BAIR: I'm feeling really confident about those three significant digits the more we talk. It's getting --

MR. FAYE: All right, I'm glad of that.

DR. BAIR: How about slug tests? Did they do slug tests in any well?

MR. FAYE: Ton, tons of slug tests. And --

DR. BAIR: Have those been processed?

MR. FAYE: -- here, you can see.

DR. BAIR: Sixty slug tests.

MR. FAYE: Sixty slug tests, yeah. We have processed those now. This probably means that there were originally somewhere between 150 and 180 slug tests.

DR. BAIR: You didn't believe?

MR. FAYE: I didn't believe them so I got it down. Sixty I can believe.

DR. BAIR: Thank you.

DR. DOUGHERTY: Bob, one quick question on the confining units. Are there data from the IRP program whether direct sampling of the fine grain materials or grain size analysis?

MR. FAYE: Lots of grain size analysis, yeah, many, many. And a lot of those were converted into a hydraulic conductivity value, but I didn't use those.

DR. DOUGHERTY: For fine grain materials --

MR. FAYE: For whatever that permeable unit happened to be.

DR. DOUGHERTY: Got it. Thank you.

MR. FAYE: But I'm very dubious of those, of that methodology, and I didn't use any of that here.

DR. HILL: You may not have used the values, but did you use the trends? Are there any trends evident?

MR. FAYE: I didn't look at trends in terms of percent fines at a particular point, percent coarse at a particular point. Haven't got to that point yet, but we can easily do that. My hunch is that on a macro scale it's probably not going to be much.

The trends are, these aquifers in terms of their hydraulic characteristics and in terms of their lithologies appear to be highly consistent until you get down to the what I call the middle Castle Hayne aquifer.

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And then the lower Castle Hayne aquifer is a big jump downward in terms of the horizontal hydraulic conductivity. It's much smaller than the younger units.

DR. HILL: This is a report that I'm sure you've seen. It's the Cardinale.

MR. FAYE: Cardinale Report, yeah.

DR. HILL: One of the figures would suggest some trends. I mean, if you take out the highs and lows and kind of look at the trends so I was surprised to hear you say not.

MR. FAYE: I didn't say there weren't any trends. I'm just saying I haven't gotten to a point where I could investigate that situation yet. There may be a trend out there. I have to say though that I'm surprised that there would be based on what I know about the lithologies, but it easily could be. It could be.

DR. HILL: Well, okay, now, I'm surprised to hear you say that because one would think that there would be archaic channels that came through and that you would expect to see --

MR. FAYE: Are you saying trans-vertically or within a layer?

DR. HILL: It could be either, but I was thinking horizontally at the moment, but it could be both.

MR. FAYE: Yeah, there are, these layers, many of them have been, they were erosional surfaces. They were transgressed by streams. And then those channels were later infilled with channel sands.

But those streams from what I've seen in the Cardinale Report and from other reports that address that, these streams are not particularly large and so if you're, and so it's sort of a shot in the dark whether a particular sample was collected in an infilled channel or in a, for that particular horizon, a relatively undisturbed area. So that's just not something I can fully address in a meaningful way.

DR. CLARK: Robert, I think I'm going to have to move on.

MR. FAYE: Okay, you're the boss.

DR. CLARK: I don't know about that. I doubt that.

MR. FAYE: This, again, relates almost exclusively to the IRP sites that we talked

about, and these are the sites that are addressed in the Soil and Groundwork Contamination Report that's in your three-ring binder. Again, don't ask me what tab because I don't know.

This shows basically the site names and the area of exposure based on the monitor well distributions at the particular sites.

And this is what I was talking about, Lenny. If you wanted to actually look at data density, this is what you ought to be looking at in terms of the areas of interest.

And this is what we call the landfill area, the northern part, Site 88, and the Hadnot Point Industrial Area. Those are the three major areas of groundwater contamination or at least the contamination of interest to us from the IRP sites.

This shows the density of the sampling points where we have samples for, that were analyzed for PCE, TCE and their degradation products. And that's pretty much exclusive. I mean, if they analyzed for PCE, they go through the whole enchilada of degradation products.

DR. BAIR: Excuse me, Bob. That map is showing wells, not aquifers.

MR. FAYE: Exactly.

DR. BAIR: Okay.

MR. FAYE: We'll get to the aquifer part in a minute. Bear with me.

DR. HILL: I'm sorry, also that's just PCE.

MR. FAYE: That's PCE.

DR. HILL: But there was, I thought at Building 820 in the Hadnot Boulevard area, just a little cluster on the bottom.

MR. FAYE: Right, it's right here.

DR. HILL: There was BTEX-free product there.

MR. FAYE: Just give me a chance, Mary. Give me a chance.

This is TCE, same idea. Those are the wells where we sampled for TCE. Here you go, Mary, that's where we show benzene. This is the site that Mary was talking about, 820. Of course, all of these concentrations I should have pointed out use a concentration range based on the size of the point that was used on the map.

And if Mr. Clark will bear with me

here, I'll go back and point that out. I'll point out Site 88 here, which is a site of major PCE contamination and also PCE contamination here and PCE/TCE contamination here as well as a lot of TCE contamination in the HPIA and major BTEX contamination within the HPIA as well.

This might address what you're talking about, Dr. Bair. This is our PCE concentrations, our PCE sampling points at depth along a section line -- this is very gross -- that runs basically from the New River over toward the landfill area, New River Site 88, Industrial Area West, Industrial Area East, and the landfill area. This gives you a notion of the depths that were sampled. So you're looking at, in terms of our identified aquifers and confining units, you're looking at that sampling that was actually all the way down to the middle Castle Hayne aquifer here.

DR. BAIR: Yes, I had a couple questions about that if you don't mind.

MR. FAYE: No, I don't mind at all.

DR. BAIR: Is the geology along A Prime consistent enough to draw some of the

formation tops and bottoms and label that?

MR. FAYE: Oh, yeah, we actually have for each one of the units that's listed in, what, Table 14?

DR. BAIR: Yeah, that ^ [report -ed.]is really hard for me to digest.

MR. FAYE: Yeah, the data report?

DR. BAIR: It really helped me because I'm just getting used to this. If you would add some of the geology on.

MR. FAYE: Well, I apologize. We actually have contour maps of the top and the thickness of every one of those units that ^ [are in -ed.] the model.

DR. BAIR: And then the question I had is probably going to come up on this one, and I'm going to anticipate your next slide and your next slide. That is you have a lot of hits of PCE/TCE very deep.

MR. FAYE: Well, let's look at that for a second.

DR. BAIR: And does that go back to --

MR. FAYE: Those are the samples where we actually had a hit above detection limits. That's TCE at the same sites that are here,

okay? And these are the places where we actually had a hit above detection limits. These are the samples.

See, you can see there's actually a fairly decent reduction from the total number of samples to the samples where we actually have a defined concentration. But the distribution with depth is pretty much the same, but these are the hit sites.

DR. BAIR: Can you go back one? I'm even more confused now. So the yellow-colored pluses and dots within the circles, those are --

MR. FAYE: The yellow crosses.

DR. BAIR: -- below your detection limit.

MR. FAYE: Those are below detection limits, right.

DR. HILL: Could we draw a distinction between reporting limit and detection limit? Because you've got a measurement at those pluses, it's just below, I mean, detection limit sort of implies that you couldn't even measure it. You have a value there.

MR. FAYE: No, that's not what it implies at all. That's the way it's reported. If you

look on the tables again in -- god, I've got to repeat this a lot -- if you look on the tables again in the Soil and Contaminant report that's in your three-ring binder that I wrote, you will see that the analyses will say something like, there'll be like less than 0.5 whatever it is. Well, that 0.5 indicates the reported detection limit for that particular sample, for that particular analysis, and it means less than.

DR. DOUGHERTY: No, no, there's great variety from laboratory to laboratory on whether that means a method detection limit, a sample quantitation limit, which is a sample-adjusted method detection limit for media and interferences, or whether it's a reporting limit, which is a laboratory^ arrangement between a client and laboratory, where do I report. And the point is not to say that we know which of those it is.

MR. FAYE: Well, I do know which of those it is. I've looked at dozens of these reports, and I'm telling you that that is defined as a detection limit. Now, there is also a few quantitation limits. Now if the person who

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wrote the report didn't understand the distinction that you just made, then I can't address it. But those are reported as detection limits.

DR. DOUGHERTY: Are these laboratory reports or engineering reports?

MR. FAYE: They're what I would call site assessment reports written by consultants and they include the laboratory, they actually include, most of the reports actually include the raw data output from the laboratory. And that has a whole bunch of abbreviations that qualify the various concentrations and they say detection limit, and that's what I say here.

DR. BAIR: Bob, if you don't mind, I'd like to pursue this a little bit. If you were to add the geology on there, one of my questions in getting to, say, some of the yellow pluses and other things is, does that sample represent a 50-foot screen, a 20-foot screen, a ten-foot screen? Does the screen go across multiple aquifers?

And, if so, this could be telling you which are poor calibration targets for your

model and which are strong calibration targets because you don't want the sample from a commingled well. You want to limit it to the shortest screens that correspond to your layering in your model.

MR. FAYE: That's right.

DR. BAIR: And then that gets back to Dave's question about the construction of the wells and whether there was grout in there or whether the ~~titecs*~~ [detects -ed.] or whatever small notations are, deep, whether that's just coming down the well bore. And I think that's critical to your setting up calibration targets.

MR. FAYE: Well, almost all of these wells that you see here that are represented, are monitor wells. I would say that the vast majority of them have a screen interval of between ten and 20 feet. That doesn't worry me a whole lot in terms of identifying a particular contributing unit except, it doesn't worry me too much for PCE because of the -- and the sampling procedures are generally well described, particularly after about 1990. So we know that they evacuated

five casing volumes et cetera, et cetera, et cetera.

What it does bother me though is with the BTEX analyses because these are monitoring wells. The BTEX that's there is sitting in a, probably in that most upper cylinder, actually has ~~three-phase~~ [free phase -ed.] in a lot of cases in that upper cylinder. So rather than sampling a four- or five-foot interval, they're sampling the whole ten-foot or 15-foot interval. So, yeah, you have to qualify that somehow. I'm not sure.

Later on about 1998, 2000, they actually started to recognize that problem with BTEX, and they shortened up their screen intervals to about five feet. So those analyses are a little more reliable in terms of what was actually there.

DR. DOUGHERTY: Quick question on that. Do you know if their protocol was if they found ~~three-phase~~ [free phase -ed.] in the monitoring well, they did not sample?

MR. FAYE: No, no, what they did if they found ~~three-phase~~ [free phase -ed.], they adjusted their water level measurement and --

you know, I don't know. I know there's a --

DR. DOUGHERTY: 'Cause it may be censoring some of your data.

MR. FAYE: I think...

DR. DOUGHERTY: And at a number of sites where if they find ~~three phase~~ [free phase – ed.], they're not going to sample part five.

MR. FAYE: You know, just looking at it, they had a lot of sensitivity with respect to the water level measurement, but I believe you're right. I don't recall a lot of analyses at the sites where they actually found significant ~~three phase~~ free phase – ed.]. I think you're right. Yeah, that was part of their protocol.

MR. HARDING: So high concentrations are going to be underrepresented in some sense?

MR. FAYE: Yes, right. But the saving grace at those sites is we do know the thickness of the ~~three phase~~ [free phase –ed.] so we're in shape there.

DR. BAIR: Bob, before you move on, there's a high correlation between where you looked and where you found TCE, which isn't too surprising, but if we look at those deep

occurrences there, and if you just go look at the section, it does go fairly close to two of the water supply wells there. There are ^ --

MR. FAYE: Oh, more than two.

DR. BAIR: Okay, and so the question is, maybe you can answer this, but I've thought we were talking about the monitoring wells. But the question is does the proximity to one of the supply wells lead to a --

MR. FAYE: Oh yeah. I think I addressed that in the report as well. And in particular with respect to the BTEX, which my understanding of the situation is if the BTEX is left to its own devices, it's just happy just floating up on the water table.

And when you find it 150, 200 feet in the subsurface near a relatively, in relative close proximity to a pumping well, why, you've got the vertical gradient -- now the vertical gradient's caused by that pumping. You've got advection, and that's what's forcing the BTEX way down into the subsurface.

And I do -- of course, the PCE being a ~~D-NAPL~~ [DNAPL -ed.], it wants to migrate vertically downward. But when you look at

these depths, particularly in the landfill area, I think you're looking at a lot of influence from HP-651, which we talked about earlier.

DR. BAIR: And I was actually, I probably inferred it too much. If the supply wells are as Dave indicated, that you can get water moving along the outside of the annular space, and this supply well is off and 651 over there is on, you could be pulling contamination from shallow to deep through the annular borehole in one supply well going to another just because it can communicate hydraulically across that.

MR. FAYE: I think that happens and also as well -- no pun intended -- you get like 651 is right in here. I think, what is this, 653, 610. Six-ten is down here. You have these wells. They may not be pumping in a, at the same time, but they're moving that mass around at depth between each other all the time every time they're operating.

This goes back to, I think, what Peter was talking about in terms of how these operations affect the simulated concentrations

that we would actually find, the actual operation 12, 16 hours a day versus some stress for a whole month, that type of thing. And we can test that.

DR. DOUGHERTY: Just a quick thing on this section since I can't put together the nearby supply wells with this cross-section.

MR. FAYE: Well, I can tell you there's a lot of supply wells here that surround the perimeter of the HPIA, and I'm saying at least a half a dozen or more that were active over time. And in the landfill area the most direct influence would have been HB-651, but there's probably three or four other wells in that general area or even immediate area that perhaps affected the vertical distribution.

DR. DOUGHERTY: Was this a cross-section showing all of those projected?

MR. FAYE: All of those what?

DR. DOUGHERTY: So all of the landfill area wells are projected onto this thing?

MR. FAYE: Yes, they are. You can see, you know, it's a gross, it's an informational slide.

DR. DOUGHERTY: That's fair once I

understand it. And again, just for information, what is the screen of these water supply wells?

MR. FAYE: HB-651 would have been and screened in at least two intervals below land surface.

DR. BAIR: I've got it right here.

MR. FAYE: Okay, there you go. I just hated to say you could look on table so-and-so.

DR. BAIR: No, I've got it. It's minus 93 to minus 103; minus 108 to minus 155 and minus 157 to minus 19 --

MR. FAYE: And those are intervals from land surface.

DR. DOUGHERTY: I have a different number from Table C-3 for 651. It's 125, 135, 140, 155 ^[, 189, 194 -ed].

MR. FAYE: In the table it's depth below land surface.

DR. BAIR: My only point was to demonstrate for others who are not so ground-watery (sic), roughly where the screens are in this cross-section tend to be 150 feet down so they're down below where we're seeing the hot spots, yet those are providing high concentration

water to the treatment plants. So there's got to be some way to get from those hot spots down to there to the wellhead.

MR. FAYE: That's just the vertical gradient's caused by -- in my opinion, that's largely due to the vertical gradients caused by pumping at the supply wells and within the radius of influence of that pumping.

DR. HILL: You have five measurements at depth and of those two are hits. And if you think proportionately to what's above in terms of the proportion of hits you have two non-detects, it's actually pretty similar or perhaps a greater proportional concentration at depth. So the fact that you're not getting that many hits might just be because you didn't look. There's no indication in that data that the water in general at that stratum is any less polluted than what's above.

MR. FAYE: Well, that's exactly right. There's a lot fewer sampling points down here than there is up here, maybe by as much as a ratio of five to ten to one.

DR. HILL: Right, the ratio of hits is actually as high.

MR. FAYE: Well, yeah, okay, okay. And the obvious reason is they were looking for contamination at shallow depths, later on got kind of surprised they found it at a deeper depth, but they had a much greater density of shallow monitoring wells versus their deep monitoring wells.

DR. HILL: I just wanted to make the point that there's no indication on this data that it isn't as polluted at depth as it is --

MR. FAYE: That's exactly right. I would totally agree with that.

DR. ROSS: Were there no deep hits below the, what I call the DNAPL site, Site 88, or is the key just covering up what might be there?

MR. FAYE: I think, Dr. Ross, the key there is that there just were no deeper wells.

DR. CLARK: Can we wrap it up?

MR. FAYE: A few more to go, and that's why we're here, right? There's the PCE now. Those are the hits. Now, as Dr. Bair alluded, he anticipated what we were going to see here. You have the PCE contamination. This is every sample including the non-detects, and then

here's the detects, and it shows the maximum and minimum concentrations that we found. And all of these questions that related to the previous two slides relate to this. Here's benzene.

There's the whole enchilada, and there's our hits again at depth. And here you're seeing that the HPIA where there was a massive benzene spill, a lot of surface contamination. Actually, now from the LUST reports we know that this contamination actually goes a little deeper down, around 150 feet. So there you see that.

There's our major plume systems that we've identified. Now this will change when and if we get into the LUST reports there's going to be a major plume of BTEX up here, probably another one right in here, definitely a big mess in here in the HPIA. So that will, we'll accrue a few more plumes when we look at the LUST data in detail.

Hopefully, this next slide says questions.

DR. CLARK: Jason, are you ready to go?

(no audible response)

DR. CLARK: Okay, Jason's up next.

DATA ANALYSES -- GROUNDWATER

WELL CAPACITY AND USE HISTORY

MR. SAUTNER: I'm just going to give a brief description of how we constructed the well capacity histories and I want to thank Bob ahead of time because I think a lot of the questions the panel will have ^ asked them in the ^. Louder? Okay.

Basically, just the well capacity history is essentially a timeline without lulls operated at the capacities from when they were put in service to the time when they were terminated or permanently taken out of service. Information we have for well capacity histories, we had over 100 supply wells that we were dealing with at the Hadnot Point-Holcomb Boulevard large distribution system areas.

Basically, we obtained a well packet of information for each supply well that contained driller logs, well capacity tests, well construction drawings, operation records, various other miscellaneous sources of information. We also had several other documentation sources examined.

We had well data lists, raw water supply lists, building dimension lists, operational records, water level tables, transmittal and correspondence letters, numerous CLW documents and various published reports. And on top of that we also obtained the daily logs for well pumps, which everyone's been discussing, as the 1998 through 2008 daily status of how wells were operated on or off.

This is just a figure of where the well locations are throughout both systems, throughout both areas. Now, here's an example of well capacity history. This is for HP-633. This is constructed for each of 100 or more than 100 wells basically just gives a date, capacity and operational status and a data source.

So for the date that we have, the date when it was put in service. We have the capacity at certain dates throughout when it was in service; the operational status and whether it was in service, out of service or when service was terminated, and then the data source of where that information came from.

And you can see where all these blanks are in capacities; we just simply didn't have a capacity given for that source of information. So that would be carried down in time, so that'll be carried down to the following empty block. This one here will be carried down to the bottom, too, and so forth.

The daily log for well pumps, simply just a scanned sheet for each month, for each well from 1998 through 2008. So it's a lot of information. There's I believe over 10,000 sheets. And the main two columns we're interested in are when the pump was on and when the pump was off. And as you can see for, this was just for January 1999 for HP-633, it was only on for the first seven days, and it was off the rest of the month.

And what we did was we used the ^ determine well capacity on monthly adjusted capacities. So from using these where we obtained the number of days it was operating each month along with the well capacity at certain times from the well capacity history, we created these tables.

This is just for all of 1999 so let's

focus on the first column or first row here first. This is January of '99. We know from seven days right here, add up the total number of days. We have a capacity of 205, which came from down here, the well capacity history.

From that we computed the gallons pumped per month. We know the total number of days in the month, from that we can get the adjusted capacity. So assuming that this well was pumped 31 days a month, instead of pumping at 205 gallons per minute, it would be pumping at 46.3 gallons per minute. And this could be computed for each well from 1998 all the way through 2008.

This is just an example of the number of days it was operated. The reason the time period is from '98 through 2000 is because the well was taken out of service or service was terminated in October of 2000. For several of the other wells we will have a full ten years of data on the number days that it was operated.

One thing that we're considering exploring doing is actually -- and this was

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discussed during Bob's presentation -- is actually taking our known number of days for a certain period of time and trying to sort historical trend back in time for a study period from '68 through '85.

There's different ways we're going to look into doing this, and we'll be using this trend, also using, we know our total average, our total annual rates from '68 through '78, '68 through '85 as well. This is a slide that Bob also showed showing you the available pumpage data. So basically, by using this '98 through 2008 daily data, we're going to try to back track and try and fill in the gaps between all these type of data time frames, taking '84 all the way back through '68.

And just to summarize it we had more than 100 supply wells. There's a lot of information to review in order to create a well capacity history for each supply well. And information for the past ten-to-15 years is more detailed than information for 50-to-60 years ago, obviously. And again, we're going to explore ways to find historical trends of how that well was pumping on a monthly basis

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using the detailed daily information as well as the annual information that we have.

With that I will give up to questions.

DR. GRAYMAN: Can you go back to slide number three? That variation in capacity, do you think this represents some changes in the, intrinsically in the wells or do you think there's some of that significant uncertainty between the tests?

MR. SAUTNER: I guess it would be really depending on, well, most of this information came from well capacity tests. They were fairly consistent in the way they conducted them. I'm not really sure as to what variation, what would be the cause of the variation.

DR. GRAYMAN: Without looking at the dates I mean you see a change from 221 down to 159, but that's an eight year period so that makes some sense.

MR. SAUTNER: Nineteen sixty-nine to '77.

DR. GRAYMAN: Can you go to the next slide? And there's a column over near the right where it says time checked. Do you know anything about the operation where they operated, they

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tend to be operated on a daily basis or was there a particular time when they checked it to see whether it was on or off?

MR. SAUTNER: I believe they -- this slide came from Camp Lejeune here -- I think they had a certain time of the day where they would send a [well -ed.]person out, and they would check the wells and report back. I'm not --

DR. GRAYMAN: When you say check, would they turn them on or off? I mean, did the wells tend to stay on for 24 hours?

MR. SAUTNER: I don't believe -- oh, yeah, that's, we did ask that question. If the pump was on, it was on one day. And if it was on the next day, it was on the complete time. So for day one to day two it was on for that whole 24 hours, yes.

MR. HARDING: I think this may, it raises this point. I know I've flogged this horse a lot, but there's a difference here between what you're going to do for the groundwater modeling and what you'll have to do for the water distribution modeling. Because while your stress period's a month in the groundwater model, the way that contaminants

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behave in the water distribution system during these interconnection events is going to be very dramatically affected by what pumps you assume are operating and the hourly, you know, flow rates.

In other words a pump can't run at an average of whatever it was. I can't remember the numbers but the average amount. It either runs on or it runs off. And if the contaminated well is on, it's on all the way, and then the contaminants can move out into the system during times of low demand or perversely in this situation, when the high demand comes on the golf course, that's when that interconnection opens up and that tends to have it move further in the system. So you can't use the same approach, I just want to caution, for both water distribution and groundwater modeling.

MR. SAUTNER: Right, and just to clarify, all of these supply wells pump directly to the water treatment plant. So we are going to be --

DR. GRAYMAN: They all pump directly to the treatment plant.

MR. SAUTNER: They don't pump into the system.

DR. POMMERENK: I think the wells that pump into a manifold collection system, there's a difference. They don't all pump against the same head. So depending on what combination of wells is on, the actual flow rate that is delivered by the well pump may vary as well. So it's just some added complication. I think one of the earlier figures you clearly saw that the wells had essentially streamed on a large water collection main. And depending on the size of the thing, I guess somebody would do a hydraulic calculation to see how well operation would affect the head ~~at each pump as it pumped~~ that each pump pumped -ed.] against, so just as an additional caution.

MR. HARDING: So another clarification, is there a booster pump, is there a storage tank and then a booster pump at the water treatment plant that then sets the grade line for the water distribution system?

MR. SAUTNER: Yes.

MR. HARDING: So there, and there's an unpressurized storage tank then at the water

treatment plant and -- okay.

DR. KONIKOW: So if you go back to the previous slide, again, I agree. There are many sources that there are uncertainty in this, but what I want to look at here is filling in the gaps. Between your data points you had implicated that like from '69 we have 221 to 1977 we have 159. You would use a 221 the whole time.

MR. SAUTNER: Yeah, or one way to do it would be maybe to do a trend and step it down.

DR. KONIKOW: Which did you do? What are you doing or what should be done?

MR. SAUTNER: This is the information going to the generator and it hasn't been used as input.

DR. KONIKOW: So that's not in the groundwater.

MR. SAUTNER: Correct.

DR. CLARK: We have a swift comment from the audience.

MR. WILLIAMS: Yeah, I just wanted to clarify that the 24-hour pumping, which would only be indicative of the Hadnot Point wells, not at Holcomb Boulevard.

DR. CLARK: We're going to have to move on to the next presentation.

DR. GRAYMAN: Can he just clarify? Well, the Holcomb wells, how were they operated?

MR. WILLIAMS: Something less than 24 hours.

MR. SAUTNER: I think they were automatic, correct?

MR. HARDING: Did the Holcomb wells pump, did they pressurize the system or was it a similar situation where they pumped into an unpressurized storage tank and then were boosted into the --

MR. SAUTNER: It's the same situation.

DATA ANALYSES -- GROUNDWATER MASS COMPUTATIONS

DR. CLARK: Okay, Mass Computation.

MS. ANDERSON: I'm going to talk at you about the subsurface mass computation and make it very brief hopefully. This is a quick overview. I'm going to recap the site locations. I'm going to highlight some groundwater contaminant statistics and outline the purpose, scope and proposed methods for a mass computation and then finish with an illustration of a mass computation for TCE.

So you've seen this map a couple of

times already. I just wanted to recap again the IRP sites, the Installation Restoration Program sites are outlined in the dark red. The orange outline shows scenarios that we talk about a lot, Site 88, the landfill area and the Hadnot Point Industrial Area or the HPIA. That's where we're finding a lot of contamination, particularly the PCE and TCE contamination.

So I wanted to emphasize some relevant numbers for the groundwater contaminant datasets. Our available contaminant data span about 20 years from 1984 to 2004. We have over 2,400 groundwater sample analyses for PCE, TCE and their degradation products. We have over 2,600 groundwater sample analyses for benzene and related compounds.

And I've listed some maximum detected concentrations in groundwater there in micrograms per liter. Of course, the PCE level at 170,000 micrograms per liter, that's at or above the solubility limit depending on what reference you use. That detection was at Site 88 where we know there was some pre-phase product in the past.

So our primary purpose for contaminant mass computation is to provide really a starting point and a lower limit for a mass loading parameter when you do the fate transport modeling. The mass estimates will also be helpful in assessing plume stability over time, and we can look at those numbers to compare to other similar sites as well, but our primary purpose is for the mass loading parameter for the fate transport model.

For this work we're going to focus on PCE, TCE and benzene for mass computations. We're going to primarily compute the dissolved phase contaminant mass. We do have some data for some areas for the unsaturated zone and free product areas that we may address with some computation but primarily the dissolve phase contaminants. And we will be looking at multiple areas across the study site.

So this slide kind of outlines our general methodology, proposed methodology starting from the left there to select and prepare the contaminant datasets from the point data that we have. We're going to develop two-dimensional horizontal

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concentration grids that represent the horizontal distribution of contaminants using interpolation techniques to generate those.

And then we'll calculate the average contaminant concentration across these horizontal plumes. And finally, we'll calculate contaminant mass by combining that average contaminant concentration in a horizontal distribution with information we have about the aquifer porosity and the vertical extent of the aquifer where these contaminants occur. That's kind of a general depiction of our methodology.

DR. KONIKOW: So is the goal to estimate the mass in the system at one point in time or as an initial condition? Because contaminants are released over some long period of time. And so I'm wondering how does this relate to what you're going to put into the model?

MS. ANDERSON: Sure. I think that's part of the data exploration that we have to do. Obviously, there's a sort of a temporal distribution to the data that we have to look at and kind of slice it in different ways and look at what makes sense, and then look at

those calculations and decide what makes sense to put into the model. So it's kind of a number of steps there that will be involved in the whole mass computation and then entering into the model. Maybe the next slide or two will explain that better.

DR. BAIR: I have a question, too. You're looking at aquifer thickness and the concentration in each one of the aquifers and then summing them for a grid block looking down?

MS. ANDERSON: There may be some other slides that explain that a little better, but yes, this process, I mean, essentially when we had the contaminant data -- and you saw in some of Bob's slides the vertical distribution -- obviously, when we derive horizontal representation of the distribution, we've got to look at a single aquifer and just only collect the data points for that aquifer, do an estimation, extend 3-D the calculation over that aquifer, and that would be a mass for that aquifer. Another aquifer would be a whole 'nother of that process repeated and then add --

DR. BAIR: Right, well, my question is that are you doing this just for the aquifers? Because the confining layers have mass in them, too.

MS. ANDERSON: I think, yes, that's a valid point, and we can look at --

DR. BAIR: And they are as thick as the aquifers in some places, and their porosity probably is not too different. So my question actually gets at porosity. Are you using a uniform porosity across everything?

MS. ANDERSON: Right now, the illustration I have here, I'm just talking about the porosity for one aquifer that we're looking at. But I think we do need to refine that and kind of look at different aquifers, different porosities if we have the data. Clay units, we have some data based on Site 88 investigations for porosity there.

So I think that's a valid question, and that's something -- it's really going to be data driven. Where we have the data and then what can we extrapolate from there and how can we extend that knowledge.

DR. BAIR: It also should be put into the

sensitivity analysis, and that's the sensitivity of the source term and the release of the source term, the concentration and timing of the release of the source term.

MS. ANDERSON: Yeah, and I think as we explore the data and kind of do some of those vertical plots that Bob has shown in his presentation, we can get a better sense of where we have to go with the other steps, the other sensitivity analysis.

DR. BAIR: But that's my point is the plots that Bob showed are all biased towards the permeable intervals where they've done monitoring wells, and the contaminants exist in between sampled intervals, otherwise they wouldn't get down to the deeper parts.

MS. ANDERSON: Actually, I do have one slide where we can maybe explore that a little bit more and kind of talk about what you're getting at I think, but we're welcoming the input and how we should approach that.

DR. HILL: In step two considering the thickness you're using as the whole aquifer thickness that you're not making slices through it, it seems odd to me in step two not

to do a 3-D interpolation of the data. I mean, there'd be no reason not to at that point, and then integrate, I mean.

MS. ANDERSON: Again, it's kind of data driven. There's a slide --

DR. DOUGHERTY: It's Surfer driven.

MS. ANDERSON: Surfer driven? We actually did look at some 3-D interpolation with GMS, and I think -- I haven't explored it yet -- but ^ with Surfer does some 3-D interpolation. And I think that it will be good to kind of run this method and then do some other comparisons with other tools to look at those types of interpolations.

DR. HILL: So when you do step two, obviously when we saw before, we had high concentrations and then low concentrations. What do you use as your point value in 2-D space given that you've had all this variation vertically?

MS. ANDERSON: Give me a slide or two.

DR. HILL: Sorry.

MS. ANDERSON: As Bob said, Mary, hang with me for a second. We'll get there.

So I just wanted to present a few

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details about the data preparation and interpolation, which obviously we're talking about. We need to select the datasets and sort of group them based on some considerations. The horizontal distribution, and that's kind of picking areas across the study site that will isolate and do calculations.

The vertical distribution, which we discuss a lot. The sample altitudes and what we're going to consider as datasets for doing those horizontal distributions. And then the temporal distribution we need to isolate sort of or aggregate some datasets based on the temporal characteristics of the data.

When we do the interpolations, we'll have to look at multiple detections at the same location and kind of generate a single value. I think it makes sense, typically we'll be using the average value, but there may be some occasions where maximum values are appropriate for that.

The non-detects and the censored non-detects for the calculations I'm showing you here, I set those to zero. Now, we can

consider different schemes for that if necessary, but by censored non-detects I mean the data that are less than whatever stated reported value, less than five, less than ten.

Non-detects, literally there are reported values that are just ND, and we have no reporting or quantitation limits to go off of on that data. So that's what I'm talking about, those non-detects and censored non-detects.

DR. DOUGHERTY: Just for those if you have a non-detect and a nearby close detect, do you somehow take into account that the non-detect may not be representative? I'm thinking about from the regulator side, of course, and from the other side you want to say well the other one's an outlier and it's a laboratory problem.

MS. ANDERSON: I think we're not to that point yet, but that's certainly a refinement that could be made. Initially, we're dealing with a very large dataset even when we isolate it to one location or area of the base. So that's certainly something we can consider and kind of refine that non-detects and censored

non-detects to assign some values or discard data that we don't feel are appropriate.

DR. POMMERENK: Actually, with setting them to zero you would, you know, whatever your statistic is that you would use to represent the total mass and then you would underestimate the, that statistic was set down to zero so you may want to consider using some type of robust regression to -- you don't actually assign values to the non-detects, but you compute your statistic on distribution of values based on that there are values. We just don't know the numbers. And --

MS. ANDERSON: We have the ~~HASL*~~ [Helsel -ed.] text, and I think that that is something --

DR. POMMERENK: Yes, the ~~HASL~~ [Helsel -ed.] text will help you --

MS. ANDERSON: -- yeah, that we can consider after we do some baseline using this methodology. I think it would be good to sort of try to incorporate the non-detects in non-parametric methods and sort of try to do some analyses that way.

For the interpolation schemes kind of

looking at, we've explored some different options for that as well, but I think we'll probably just use the ordinary pre-game using standard default assumptions in Surfer Software. We did explore a little bit the autofit ^ [semivariogram -ed.] ~~gram~~, compared that to standard default assumptions in Surfer, and they seem to come out very similar for the mass computations, but that's something we can continue testing as we move forward. For the calculations that I'm showing here -- in our initial runs through this we're using ten foot-by-ten foot grid cell size.

So I kind of want to go through just a quick illustration, and it is just a slice, just a subset kind of illustrating the approach of the mass computation method. This is for TCE. This is the map that Bob showed as well showing the distribution of TCE across the study site. It's concentrated in a couple of different areas there.

We're going to focus for this illustration just on the landfill area. And this is the temporal distribution of data that

we have for the landfill area. You can see in the middle there, there's the extraction well start up in October 1996. We have some data before that, a good bit of data after that.

For this illustration again I'm going to kind of look at this pre-extraction well start up database 1984 to 1993 and do some calculations with that. Certainly, we can run calculations with the first few years after extraction well set up or start up because there's very low flow with those extraction wells, and we may be able to use some of that contaminant data in a more extensive monitoring well network that was in place to do some mass calculations there.

DR. DOUGHERTY: Just to clarify, this is a remediation extraction well as opposed to a water supply --

MS. ANDERSON: Correct.

DR. DOUGHERTY: -- extraction well.

MS. ANDERSON: Yes. That's one, the remediation wells, the extraction wells were put in place in October 1996, when they started cleaning up the site.

So I'm going to focus on that earlier

data range there. And this is the vertical distribution of TCE in the landfill area just for that selected time frame that we're looking at, 1984 to 1993, so it's a little bit, it's like the slide Bob was showing, but it's a little more refined just to include the selected dataset.

I have included off to the left there just some general kinds of boundaries for the different aquifer systems: the Brewster Boulevard, the Tarawa Terrace aquifer and Castle Hayne aquifer system. And these are very general. They're kind of averages of top elevations and thicknesses across just the landfill area. So I haven't extended it across because there obviously are local variations. We're still dealing with a pretty large area so I just kind of added that guideline on the left-hand side there.

So you can see with this vertical distribution that we have data, contaminant data, just for two different aquifer systems, the Brewster Boulevard, the upper aquifer system Brewster Boulevard and then the Castle Hayne aquifer system.

There's really no data except for that one non-detect off to the left there for the Tarawa Terrace, intervening Tarawa Terrace aquifer system. So it's a constraint of the data for this time period. I think for later time periods we do have some data for Tarawa Terrace, that aquifer system.

But again, to illustrate mass computation, I'm just going to pick this one slice, this one horizontal slice of data in the upper Castle Hayne aquifer, the River Bend unit, and kind of run the calculation with that because I think that's how we'll have to proceed. Looking at grouping the data vertically, doing separate calculations for each and then kind of summing them, stacking them up.

So this is again, as I outlined in the general approach, we'll take that contaminant dataset, the data points, and interpolate them into a concentration grid, a two-dimensional horizontal grid, and that's what is shown there on the left, a traditional contour map, planar view. On the right I'm showing a 3-D wire mesh representation of the contaminant

concentrations with the Z axis being TCE concentration in micrograms per liter.

So once we've established this concentration grid, we can use Surfer's grid volume utility to obtain both the planar area of the plume and also the grid, quote, volume, which I think this 3-D wire frame grid kind of illustrates the volume that I'm talking about; it's kind of these strange units of micrograms per liter multiplied by base area of each cell. It's essentially an area weighted concentration for each cell grid summed up to represent the volume of that concentration grid.

DR. HILL: Can I just ask a question?

MS. ANDERSON: Sure.

DR. HILL: I don't know that you can do this now, but it's really kind of critical where the points are that you're contouring, and they're not clear in that figure.

MS. ANDERSON: Yeah, the post points are not big enough there, are they? But that's something obviously we're, with our interpolation techniques kind of running interpolations and checking the post map to

try and make sure it's a good representation of the data that we have.

DR. HILL: If those ^ aren't supported. It's just ^.

DR. DOUGHERTY: Clearly, they're supported by over-fitting, I suggest.

DR. CLARK: Scott, go ahead.

DR. BAIR: Barbara, my question would be if you look at the fishnet plot on the lower right, that would be one, two, three, four units that you're representing there?

MS. ANDERSON: Aquifer units?

DR. BAIR: No, just four horizontal units. There's a horizontal line going down from the peak and then there's a shoulder off to the left, and then there's another -- those are concentrations?

MS. ANDERSON: Yeah, that corresponds to the legend over there on the left --

DR. BAIR: Okay, so how many aquifer units are within that then? One?

MS. ANDERSON: Yeah.

DR. BAIR: Got you.

MS. ANDERSON: We're just taking that one slice of the upper Castle Hayne River Bend

unit and looking at that.

DR. CLARK: Rao was next.

DR. GOVINDARAJU: I think I want to follow up on that next question. That is, this is going from 1984 to 1993, so this one unit you are computing is somehow over time, and time does not seem to factor in.

MS. ANDERSON: Right. I don't have a, we aggregated or I aggregated this data before the extraction well started up in 1996 because really if I plotted -- I have another plot and I didn't overlay it on here, but these numbers, the bar graph showed the total analyses we have, but the detections for each of these are the lower number, obviously. So if we want to just aggregate just 1984 to 1987 as one unit. There really aren't sufficient detections there to do an accurate interpolation. It would make more sense I think to use smaller time frames. But in this case there just weren't enough detections to really do a good interpolation so it's aggregated across that whole time frame. Is that --

DR. CLARK: In order to meet our streaming

video guidelines we're going to have to wrap this up. So let's take just one more question and then, Barbara, can you wrap it up?

MS. ANDERSON: Sure. But maybe not, it's Lenny's question so I don't know.

DR. KONIKOW: So then the question is how do you go, you'll calculate a mass, but then how do you go back in time and use that to estimate what the mass loading rate is over the duration of the model? The Tarawa Terrace situation you had essentially a point source with a known location and a fairly constant over time disposal rate. Here I'm not sure how you're going to reconstruct the history of mass loading.

MS. ANDERSON: Yeah, I think that's going to be a challenge. I will say -- and Bob can chime in where he sees fit, but I think that for the landfill area I think Bob has, from his expert analysis of all the data that he's looked at, has determined that at Site 88 there was a dry cleaner, same as ABC Cleaners there was a base dry cleaner. And this landfill contamination is probably tied to disposal of filters from the, spent filters

from the dry cleaning operation at Site 88, and there may be other sources. There may be buried drums, what have you, at the landfill area, but --

MR. FAYE: The issue, Lenny is basically, you know, you take what you get. We want to have a computation of mass prior to the onset of extraction. Yeah, and the data are over a particular period of time so, yeah, you had some concentration reductions because of degradation over that period of time, et cetera, et cetera, et cetera.

But I won't say the time is relatively immaterial here, but if we have this mass at this time, it basically gives us a minimum mass that we can work from. And what it is, I mean, it's basically, you know, you've got a flawed starting point or you've got no starting point. So, I mean, that's really what it comes down to. Of course, it's better to have a flawed starting point in my opinion.

DR. KONIKOW: You've had extraction wells over the whole duration of the system, but they were called water supply wells.

MR. FAYE: There again, sure there was mass

removed from the system, but still we don't know what that mass was or we have a couple of concentrations that we could maybe make some estimates, but you'd have so much uncertainty you wouldn't assign a lot of reliability to that. But here again, I mean, it's not a perfect system. It's not a perfect analysis. But it gives us a starting point which is what we're after.

DR. CLARK: Let's give Barbara a chance to wrap up her presentation.

MS. ANDERSON: Sure, really after this I'm just illustrating how we can use, there's a Surfer utility to obtain both planar area and this grid volume and we can use that to easily obtain the average TCE concentration across this horizontal plume that was generated. There's a Journal article, Joseph Ricker* published in 2008 in "Groundwater Monitoring and Remediation" that kind of illustrates this if you want more information. But that's kind of what we were following with this approach. And then I just was showing the general equation there at the top and the parameters and values that I used for this illustration.

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The first couple of values, the planar area, the average TCE concentration. Obviously, as I said, obtained from Surfer utility. Aquifer thickness. Here we're just using an average estimated thickness for the particular aquifer that we're looking at. And aquifer porosity we can look at effective or total porosity. We have some, I think, good values for that, 20 percent that was used in the Tarawa Terrace work and discussed extensively in one of the chapters in the Tarawa Terrace reports. The 40 percent total porosity just for this upper Castle Hayne River Bend unit, again, is from some site-specific data from Site 88 investigations. And we can refine this hopefully for each aquifer and each area that we're doing these calculations.

DR. KONIKOW: What did you use -- a couple more -- why did you use 22 feet for this system here when your earlier slide shows a box around it that looked like it was at least 35 feet thick where you encapsulated the data? And then the second question is why not account for the spatial variations, the elevations at the tops and bottoms? Why don't

you use Surfer to get, why don't you consider multiplying all those concentrations? And why an average thickness? Why don't you use a thickness at each grid point?

MS. ANDERSON: I think we can do that as a refinement. We can import the extrapolation we've done with the model and GMS and kind of get actual cell-based aquifer thickness. And the other about the average that we've used here, I think -- and I noticed this in your comments you were referring to the Tarawa Terrace report which I think are a bit north of our location.

DR. KONIKOW: Just go back a few slides for this location. There, that looks like a vertical interval of 30 to 35 feet that you encapsulated the data yet you're using 22 feet. That's a pretty big percent difference.

MS. ANDERSON: That's the contaminant data. When you look at the actual extrapolation of any boring location or boring data that we have, and you look at the encapsulating aquifer system, we actually have a more refined sort of estimate of the thickness based on other data.

DR. KONIKOW: Are you saying that the data points here are --

MS. ANDERSON: Right, right. I think some of these data's a question of local variation.

DR. CLARK: Let's draw this to a conclusion so we can meet our deadline. So we'll pick it up at 1:30 this afternoon.

(Whereupon, a lunch break was taken between 12:37 p.m. and 1:30 p.m.)

DR. CLARK: Okay, we're ready to start up again. Video streaming is going to be online in a few seconds. Morris has got a few things he wants to do, wants to introduce Dr. Aral.

MR. MASLIA: Thank you for that morning session. This is the type of feedback we're looking for. We had some very interesting and informative and probing questions so we're going to continue this afternoon. Just a couple of housekeeping things before I introduce Dr. Aral.

If people would like to go out to dinner other than the hotel, there's a couple of restaurants in the area. One's a little bit more expensive, a nice French restaurant. I can see if they have room. We can talk at

the next break and just see. Or if everybody just wants to do their own plans and maybe get together that's fine with me. Y'all may not want to eat with me, dinner. Actually, my wife would like to see me at home one day during the past two weeks for dinner. But at the next break maybe we can sort of formulate plans.

STRATEGIES FOR RECONSTRUCTING CONCENTRATIONS: PRESENTATIONS AND PANEL DISCUSSION

With that said, as we saw from this morning, a lot of data, a lot of information and how exactly to analyze it, how to make sense of what it is and how should we put it together so we can, if we want to, try to do a numerical model like we did with Tarawa Terrace. Questions you asked, Lenny, and pointed out, there is not a single source so where do we begin in that temporal distribution?

So after we had completed Tarawa Terrace and just looking at the surface of this, I asked our cooperator at Georgia Tech perhaps there might be a method either available or maybe we could look into developing one where we might be able to use

some of the data that's captured, the contaminant data that's captured in either our supply wells or observation wells.

And would there be from a screening level a way to avoid or minimize having to transfer the data that we have in reports and analyses to then trying to categorize it for a numerical model. Just some of the issues on assigning supply well pumpages from the scheduling that we've got versus actually putting it into the model.

And so Georgia Tech and Dr. Aral have come up with a screening-level method. It was described in the notes, but Dr. Aral's going to describe it in more detail, and again, it is meant as a screening level, but it may be something very useful for us to either proceed with that initially or provide more information from that standpoint. So I'm going to turn it over to Dr. Aral, and let him proceed.

SCREENING-LEVEL METHOD

DR. ARAL: Thank you, Morris, and welcome back. When I heard this task from Morris, I said this is a difficult task. This is not

easy to do. But then I'm sitting there and listening all of the critique that you guys are giving to the other approach, and I said my task is very simple because none of those critiques apply to what I am doing.

Our task is if we know what we know today, can we predict what has happened in the past? And then we are thinking about this at Georgia Tech where I work, and we thought, well, we do the opposite all the time as engineers. If we know what we know today, can we predict what is going to happen tomorrow? So let's look at that approach, and let's see whether we can get some insight and make some use of that analysis in predicting what has happened in the past.

So predicting the future and using the information from the future events is based on some control theory analysis. And I'm going to give you three simple examples where we use this approach and then try to extract some insight from this analysis to use to answer the question that we are trying to answer in this case.

For example, everybody has a car.

Everybody has a cruise control. You are driving down the highway, and you don't want to worry about the gas pedal. You just want to enjoy the scenery. What you do is you set your cruise control to a given speed, and you would like to watch the scenery after that. You assume that something in your car is going to adjust everything such that the system output is going to be that speed.

That's a custom control mechanism that is installed in your car. What it does it looks at the speed of the car, senses it, and then based on a computer program or a chip installed in your car, controls the system which happens to be in your case in the car, an engine, adjusts the carburetor, adjusts the system input which is the gas, so it maintains the speed. This is the simplest application of a control based analysis in our daily life.

Other applications are a little bit more complex. For example, we do, as engineers, reservoir management. We try to maintain a certain volume of water to supply the demand at all times by controlling the spillway gates. It is based on the same

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principle. In that case, of course, we have to predict the future.

We have to predict that there will be some drought season in the future or rainy season in the future, et cetera, such that based on that prediction we adjust the spillway gates. We release or retain water to keep the supply meet the demand. That's another application.

Another application is in power systems. We cannot store energy so we have to generate power at the time of use. We have to predict how many million people is going to turn off the switch in their homes and predict how many million are going to turn on and then estimate the demand at that time and then produce the energy required at that time.

All of those analysis is a time series-based analysis, and it's a control theory-based analysis. We have different ways of looking at this. We have intelligent control systems, optimal control systems, et cetera, et cetera, et cetera. This field is well established in engineering analysis.

Now what are the characteristics of

this system? In the examples that I have given the system information is known. We know how engine works. We know how to calculate the volume of a reservoir, et cetera.

What we don't know is how to maintain the system output. System input is fixed. It's today's information or yesterday's information. So what the controller does given this information on the system it adjusts the system behavior a little bit so that the output becomes what we want. So this is the basic idea of control theory based analysis.

Now, what we have here is the same system but in a reversed order in the sense that we know the system output. As you have seen this morning, there are numerous monitoring wells which are located at different locations in the site, which has been monitoring the site for the past 15 years. So the system output is known.

We don't know the aquifer properties; that's what we heard again this morning. We are trying to characterize the aquifer system.

Now, the question here is this yellow is the same yellow here, the system input. What should be the system input such that as it passes through the aquifer gives us what we have observed for the past 15 years. So this is a control theory-based analysis similarly, but the question is we are not going to predict the system output, we are going to predict the system input. That's the whole idea, and that's the only difference.

And there's one other difference and that's the following. We don't know the aquifer properties as well. We don't know how the system behaves. So this is a basic introduction to the idea, but I will go into details of the algorithm in a little bit more detail later on.

We are still in Camp Lejeune. We are looking at contamination sites at Hadnot Point or landfill area or other regions of the Holcomb Boulevard. And what we have done in the past is one of those sites, which happens to be the Tarawa Terrace area. The model that is used in this area is well calibrated, tested, applied, et cetera, and we have some

existing models that we can implement in this study.

Now let's understand how the traditional way of looking at this problem goes. It goes as follows, and you have heard this all morning. Collect the data, develop groundwater flow and contaminant fate and transport modeling. That will hopefully give you some concentration profiles in certain water supply wells in the aquifer, create a mixing model, put it into water distribution system eventually giving you the exposure pattern at the site. So this is the traditional way of looking at this problem: data, to model, to mixing model, to water distribution system analysis.

Now, the purpose of the current study is a little bit different. All these steps that we have discussed this morning, and I have summarized here, takes a lot of time, a lot of energy. There's a lot of uncertainty as you have heard.

And the question we were asked to answer is if we know the field data, and this happens to be the Tarawa Terrace Area PCE

Contamination Database, can we skip all that intermediate steps or modeling of fate and transport analysis and jump to the final step of estimating the contaminant levels in the wells without using models or the models that we use traditionally? So that's the purpose of this study.

First of all we have to immediately identify what our limitations are. How we are going to overcome those limitations. So let's describe that. As Morris has said, this is going to be a screening-level procedure. We are not claiming that we will get exactly the same accuracy level -- and some of you are questioning that already -- exactly the same accuracy level going through the process of modeling. We accept that.

The other important difference is that the proposed method is not going to be applied to the whole area that you see here, which is Holcomb Boulevard and the Hadnot Point, but it is going to be applied locally in the following sense. We have talked about data clusters, density, data density this morning. So we are going to make use of that density

and apply this method locally, to landfill area maybe, just look at that region.

Or apply it at some other source contamination where there's data, where there's monitoring stations, where there's monitoring data for 15 years, which we can use. That's the idea. So we can pick this method and apply it to different places. And as I have demonstrated in my report, we have also applied to Tarawa Terrace area creating a synthetic data to see how it works, and I'm going to discuss that today.

Other limitations, of course, quality and quantity of the data is extremely important. If we feel that at a certain site we don't have enough data, we will not apply this method. It's that simple. It doesn't work. So we have to wait for the site data analysis to be complete for us to implement this method at Hadnot Point or Holcomb Boulevard areas.

The other advantage of this is we can use this method at any of these small regions where we have some data to characterize different chemicals whether it be PCE, whether

it be benzene or TCE, et cetera. If we have a fingerprint, we can use the method. If we don't have a fingerprint, we cannot use the method. So this is the starting point in our expectations in this method.

Let's also look at the technical details a little bit. I have to go back to the same procedures that we use in our traditional approach. What do we do? Well, we use groundwater flow modeling. This is the basic governing differential equation for that system. From this we get the ^ [velocity -ed.] ~~the~~ field in a multi-layer system.

We put that information into contaminant fate and transport, and then whichever method you use, finite difference, finite elements, ~~metal~~ [method -ed.] of characteristics, et cetera, this procedure lends itself to a matrix system to solve for the concentrations at the points of interest.

Time rate of concentration multiplied by some matrix M usually called in finite element terminology mass matrix, concentration times another matrix S, usually called the stiffness matrix, and then some loading

functions whatever they may be.

So I would like you to remember this final outcome. If you go through this process properly, calibrate the model, and this and that, you end up at this stage which is not going to change after that point. This is your solution system.

This matrix equation represents the system itself after the procedures are properly implemented and the models are properly calibrated. So I would like you to remember this because I'm going to refer to this later on.

Let's also remember or look at the data that we may have at Hadnot Point. This is the general trend in the databases that we have seen so far in Hadnot Point area. Contamination starts at a zero and between T-zero and T-A, there is no monitoring of the site. There is no monitoring data, but during this period from T-zero to T-A, there is water supply wells operating at the different locations at different schedules at the site.

And then at time T-A the contamination events are discovered, water supply wells are

shut down and the sites are being monitored. So we enter a period of no pumping of water supply wells and a period of observation. This is traditionally about three or four years from T-zero to T-A, and this is about 15 years from T-A to T-F, on that range.

And at certain sites we also have some internal points which is going to be very important for us in our analysis. Not at all points these internal observation points are available, but at certain sites there is some internal data points during pumping period. So keep that data structure in mind as well.

So what are we going to do? Well, as I have proposed, we are just going to skip all that modeling. We are going to look at the aquifer system as a black-box model, and we are looking at observation well concentrations or monitoring well concentrations, which are characterized in director X of T and X1, X2, X3, et cetera, are different monitoring stations which are recording concentrations over time. So X of T at the forward time, that is, after T-A is known at several monitoring locations. And we are interested

in this time series change of this monitoring database as it happens over time. We are trying to understand that or trying to solve that.

Now, what does our aquifer system include, this black-box that I have drawn? It's not black but golden box in this case. Well, it includes everything. ^[Hydraulic - ed.] conductivities, different aquifers, advection, dispersion, diffusion, reaction, contaminant sources.

We don't know where they are, but we don't care because we are only looking at the monitoring locations. We are trying to solve everything at the monitoring locations. We are not trying to bring the contaminant from the source to the monitoring location.

What is an external forcing function that characterizes the behavior of this aquifer system that is the pumping rates at water supply wells which occurred between T-zero and T-A time period? And after T-A time period UFT is equal to zero. So those schedules we know, and actually so being characterized as you have heard this morning.

So our control theory based system is based on this black-box model, and we are trying to predict the time series evaluation of this XFT which is the concentration values at different monitoring stations at the site and not the whole Holcomb Boulevard, not the whole Hadnot Point, just landfill area, just another contamination site somewhere else in the site.

Now, this is the same matrix that I have shown you earlier. If you multiply the earlier matrix by M inverse, you get a matrix M instead of S and then as a load vector you get a matrix Θ, which is in front of this forcing function, UFT. So what is the size of this matrix M? It's an N-by-N matrix, N being the number of observation points. If we have five observation points, it's just five-by-five matrix.

What is the size of this Θ matrix? It's N-by-N. It's the number of observations times the number of pumping wells that we have at the site. UFT is the pumping schedules. X-dot is the rate of change of the concentrations at the observation points. X-

zero is the initial value of the concentration at the observation point.

It's our assumption that if we look at the start time of contamination, whatever the contamination was, it's not going to be immediately observed at the monitoring station, so X-zero is always zero to start the solution. It will take some time for the contaminant to reach the monitoring well. That's my assumption.

So if we solve this matrix equation using our forward time integration -- and just using some symbolism here which is standard -- we can write the resulting matrix in the squared parentheses here as A and)-T times Θ as B, and our step-by-step solution becomes this. So starting from time zero at K is equal to zero, we can incrementally go forward in time to solve for the concentration profiles in five, ten, 20, 50 monitoring stations, however many we have if we know the matrices A and B.

But we don't know that. And that is the system matrices that we identify as A, and this is the forcing function matrix that we

identify as B. So our task to solve this problem is very simple now. Can we determine, can we find a method to determine the matrix A and the matrix B? Well, actually, I'm introducing this as well, we can use a backward time integration process as well and look at the development of the matrices.

The outcome is basically the same. It goes backward in time from K-plus-one to K, but there are still two unknown matrices, A of B and B of B to subscript indicates that it's a backward system matrix. So backward, forward, the procedure is not going to change, and we can handle both of them.

Now, so our task now is to determine the matrix A and B. But let's look at this database. This period from T-A to T-F where we have all kinds of monitoring data is a period of no pumping. So if you look at our forward time integration scheme, U of K in that period is zero, no pumping. So our matrix becomes much simpler for that period.

If we have a time series of X of K, we should be able to determine the matrix A very easily. It's a least squares application,

very straightforward. And this matrix A characterizes the aquifer properties at the monitoring location not in a region, at the monitoring location neighborhood. That's all we care. So we have determined the matrix A using a least squares method.

Now the next task is a little bit more difficult. We would like to determine the matrix B. A is already there. It will be always there because it's already solved. To determine the matrix B we use an optimization method in the following sense, that we describe the objective function first.

This objective function says that the difference between the simulated concentrations at observation wells at time T-A or the difference between the simulated values and the observed values should be minimized. This is our procedure, objective function of our solution for matrix B.

If we're going to minimize this difference in a least square sense again subject to the conditions that this is the time series solution of this monitoring well behavior, and if we know A already, then the

only unknown is B. So this objective function through a minimization process determines the coefficients of B such that this task is accomplished as best as it can be accomplished.

So this is the optimization analysis that we use to determine the matrix B. Basically, we have used genetic algorithms to solve this optimization problem which incrementally adjusts the coefficients of the matrix B such that when we start from T-zero and start predicting the monitoring station concentrations, we end up as close as possible to the values of observation, observed values of concentrations at the monitoring stations at time T-A. That's the constraint here.

This method is that simple. We do these types of analyses as engineers routinely. This optimization method is not any different than what I have used earlier in other applications. Now, let's try to apply this to our Tarawa Terrace site and see how good we are.

So what we have done is we have used the calibrated models that we have at the

site, Tarawa Terrace, input the same mass loading at ABC Cleaners, selected a smaller region -- as I said, this applies to a smaller region -- and generated a plume based on certain pumping schedules which we knew at the Tarawa Terrace area.

We used the pumping schedules at TT-26, TT-53 and TT-67. And this is the plume that we have generated over about 40 years starting from the contamination event that has occurred at time T-zero at ABC Cleaners. Then we have selected in our finite element match or if it's a finite difference, it's a center point as well, certain points where we have recorded the data. This is going to be our observation points.

So we know what this observation point, this observation point, et cetera, recorded. We have information on the pumping schedules of these three pumps with one difference. We have stopped the pumping schedule of these three pumping wells. This is the pumping schedule for the wells that we have selected at stress period, that is month 408, and let the simulation continue after

that without any pumping at the site.

This is going to generate exactly what we expect to have data at Hadnot Point, a pumping period and no pumping period, and we will see what has happened to our concentrations. This is what has happened.

Contaminants start at time-zero and increase at these five nodes that we have selected as our observation period or as our pumping period. And then when we stop pumping at 408 stress period, some of the nodes are showing as a decrease in concentration like these, and the others are showing increase because the plume is moving. The downstream observation points are seeing more concentration over time as the plume moves downstream even if we have stopped pumping.

So this is our initial database. What we are going to do is we are going to blank that out. We don't know what has happened there. We are going to predict that part. We are going to predict that part using what, only the data points on this side. And also, we are going to predict that part using the concentrations at time T-A. Those are the

values that we have used in our optimization model. We try to reach to that point. And I think I'm going to show you some of the results that we have next.

After we determine the matrix A using the data after the pumping has stopped, we wanted to see whether our matrix A behaves nicely. For these five locations, obviously, the least squares method works. We expected that anyway. So the simulated and the reconstructed profiles after the stoppage of the pumping works very well, and the matrix A is well-defined for this region of five observation points.

So that side is fine, but when we go back now we have to predict 40 years of system behavior when there is pumping. And initially I am showing you here the zero internal points case. That is, there is no internal points that we have used in this application. Obviously, this is not that good but the trend is there.

If we add some internal points, and in this case we are adding only eight internal points out of 34 years of database, and not

eight data points on each line. It's just eight data points randomly placed, and here they are. As you can see, the objective function performs well. It just matches the internal data points between predicted and observed values very nicely.

So as you can see the data gets better, the predicted concentration profiles gets better in the pumping period. If we add just 15 points, this is what we have. So I'm very happy with this in the sense that there is such a method that we can utilize, and obviously, the accuracy of the procedure is improving as we include some internal points.

And I can do that over the weekend in terms of time associated with the task, and this is the 15 points that I have used in this case. I can look at the backward process. I'm just going to go through the slides very quickly. This is the verification of the matrix A sub B, and then, of course, this is the zero internal point backward solution.

And backward solution by that we mean we start from here and move backwards in solution to time zero, and then eight internal

points and then 15 internal points. As you have noticed now, we have two procedures, going forward, going backward. These are independent procedures.

Then we said can we link them. Obviously, if we link them this method is going to use some information from one another, and it becomes an intuitive process. And if the process converges, then we have a very good method in our hands to apply at our site.

The way we are going to use the backward/forward solutions iteratively is as follows: We know internal points improve the solution, and we know from our experience so far the forward method works better closer to the time T-A. Backward method works better towards times zero.

So what we are going to do is we are going to assign some random solution points obtained from the forward solution close to the T-A time frame as data points in the backward solution. And then use the backward solution, get some random points from the backward solution closer to time T-zero, use

it as internal data points in the forward solution. And if this converges, then we have a very good method in our hands.

So in summary, our next step is the use of forward/backward procedures iteratively to improve the solution, and we know also how to add confidence bands to the solution. We can give you plus or minus ten percent error, and we can propagate the field measurement error as well as computational error that we may have in our analysis and provide a band of accuracy interpretation over these databases. And finally, if all goes well, we are going to apply this to Hadnot Point area.

With that I will stop and answer any questions if you have any.

MR. HARDING: Yeah, I have some questions. This looks very interesting. It seems like this method will lump a discontinuous, inhomogeneous system into something more homogeneous that can make, you know, can help simplify, accelerate computational effort and things like that.

Two questions: A, you still will need pumping schedule if I understand this

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correctly. Secondly, where do the internal points come from? And this also seems to rely heavily on the initial condition that you applied here, that X at T-zero is zero. How do we know what T-zero is?

DR. HILL: Can I add one condition onto that so you can do it all at once? Also, your calibration in the non-pumping period require you to, you did it to simulated results from the original model, and so also comment on when you don't, obviously, you're trying to replace the model, and you wouldn't have simulated values. You would have the noisy measured values at that point. And it seems to me that's a problem, too.

DR. ARAL: The first question, this aquifer here is extremely heterogeneous, non-homogeneous and all that. But this aquifer here, which is the landfill area, we can very easily make the assumption that everything is homogeneous there. So that's not a big deal.

We are not proposing to apply this method to the whole region. We're applying it to a smaller area where we have monitoring data, and that is what we are trying to

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characterize. And we are going to apply this at different locations separately. So the matrix A is going to change. Every time we use this at a different site, based on the fingerprint that we have, the matrix A will change.

The matrix A will also change based on the characteristics of the contaminant as well. It's fate and transport. That's also included in the system behavior. If we have a PCE at this location, the matrix A is different than if we have a TCE at this location because degradation rates are different. The behavior of the observation points are different.

The other question was how do we synthesize the data? We are going to exclude obviously any data which we cannot predict a trend. The data that we can use in this analysis should give us a profile of some concentration over time. If it is an oscillating database, we will simply discard that monitoring database. We will not use, we will not model or we will not predict the concentration at that location. We will use

another place where we have a better data. If we have none, we will not use this method.

The other question was --

MR. MASLIA: The observation internal points --

DR. ARAL: Okay, the internal points, we discussed this with ATSDR or ATSDR group. There are some sites at Hadnot Point and Holcomb Boulevard where there is some internal data which is available. And that doesn't have to be a time series data like the one that we discussed a minute ago, after the stoppage of pumping has to be a time, a one-time observation, which is fine. So we can use that internal data if available as a database to improve our solution as I have demonstrated in the case of Tarawa Terrace application.

MR. SAUTNER: Also T sub zero, Dr. Aral.

DR. ARAL: What did you say?

DR. DOUGHERTY: Also T sub zero.

DR. ARAL: Oh, T sub zero, okay. Remember, we are looking at the monitoring locations. The T sub zero is associated with the beginning of time somewhere out there which

starts looking at the conditions of the monitoring well data. What we are assuming at that point is -- and that only appears in the forward time solution -- we are going to start this solution at a time where there was no contamination at the monitoring well.

That is our initial assumption. We are not saying year 1952 is the start of contamination. All we are saying is at 1952 there was no contamination observed. Let's start from there forward, move forward. Now, having said that, I want to point out one of my slides here, the backward solution.

Look what happens. We start from here and move backwards, and we end up with a zero concentration at this known point at a given time. The backward solution also interprets us the beginning of contamination, expected beginning of contamination at this monitoring location. That's an added information. I haven't even discussed that.

So we are not saying that we are starting at time zero as zero, but it's all zero from zero to 80 stress periods according to this analysis. So the use of backward

solution has that advantage as well.

Yes.

DR. BAIR: I may be missing the obvious, which happens a lot, in the bigger picture this is giving you concentrations at monitoring wells. How does that help with the water distribution model? Can you make that link?

DR. ARAL: Of course. If we have concentrations at the water supply wells measured after time T-A, which we do have, we can include those as our monitoring locations in our database. So the matrix A is going to characterize the water supply well locations as well.

And then when we predict, one of these lines that you see here is going to be associated with the water supply well position. So now we know the contaminant profile at the water supply well, and then we can take it to the water distribution system after that. So the monitoring locations that I'm referring to always doesn't have to be monitoring locations, but it can be water supply well locations where we have data on

concentrations between stress free period 408 all the way to, I don't know what, 600.

So that's a good question, but the information is in there if we have -- in other words, let me put it this way. We have to have concentration profiles observed at the water supply well locations to predict the concentration profiles before T-A. There are other ways to answer that question, but I don't want to go into that.

DR. BAIR: Okay, let's do it.

DR. GOVINDARAJU: Just a couple of points. In your last slide you said you were introducing Kalman filtering?

DR. ARAL: Yes.

DR. GOVINDARAJU: And so that is to basically take into account both error in observations and perhaps model error also. Is that correct?

DR. ARAL: No. We have a, it's again, when I use control theory-based analysis, we exactly didn't use the control based theory analysis. We have adopted some computational procedures to propagate random errors in data collection and errors in computation into our

matrix analysis system to create bands of confidence levels. It's not exactly like you and I know in Kalman filtering analysis. Uses the similar concept, and we are using the name there, but we are not using the Kalman filtering approach.

DR. DOUGHERTY: So you're propagating a noise vector rather than using the system matrices so you're estimating the effect?

DR. ARAL: We are propagating a noise vector in the observation database into the system.

DR. DOUGHERTY: And then presumably for dealing with the system noise, you're applying the same sort of thing. You jiggle the matrix. You get an estimate for how much it impacts the vector and create a vector and drive the original system back.

DR. ARAL: Exactly.

DR. DOUGHERTY: I have a couple, I have lots of questions, but I'll try keep it focused. One was in the presentation you talk about the source strength as one of the input factors to the gold-box system, yet the source strength doesn't appear in the matrix equations, at least explicitly. So the question was, are

there circumstances in which it needs to appear explicitly?

DR. ARAL: No, because the source is not at the monitoring locations. The source is somewhere else.

DR. DOUGHERTY: I understand that.

DR. ARAL: Right, so it is turning into the aquifer. It is moving down, and we are looking at what is happening at the monitoring locations. We don't know how much source there was, what the total mass is.

DR. DOUGHERTY: I understand, but in the same way you're using three pumping wells which are not the monitoring wells, so those things that are exogenous to monitoring are important to the system. So the question is still why does the source strength factor not appear in some way?

U is located spatially. It's not co-located with your monitoring wells, yet it's a factor in a linear system. So in the same way just because the source is some place else, it could still appear in the system.

DR. ARAL: It is. It is characterized in this matrix A. Wherever the source is,

however it was, how long it discharged is being observed in the monitoring station, A or B or C, which is characterized by this matrix A. As I said from the beginning, concentration sources, aquifer parameters, diffusion, dispersion, reaction is a black-box in here.

DR. DOUGHERTY: I understand it's a black box. They don't appear in the stiffness matrix. They appeared in forcing function, which is what you reduced to be U. So I didn't want to get into that level of detail here. I don't think it's appropriate.

DR. ARAL: The only forcing function that we think is going to influence the profile of appearance of a contaminant at a monitoring station is the pumping that was going on nearby that -- we are not going --

Okay, let me back up a little bit. Here, when we use this method in this landfill area, we're only going to use the water supply wells in this little box. We are not going to use the --

DR. DOUGHERTY: I understand.

DR. ARAL: Right. So we are only going to

look at the water supply wells near the monitoring stations, which influences the velocity field of the aquifer, which I think is important to characterize based on T-zero to T-A time frame.

DR. GOVINDARAJU: I think two points perhaps for clarification. What you are doing is you are using present data to predict past behavior. And let's say you focus on the landfill, and you only look at data in the landfill region. So there is an assumption that whatever let's say was happening in Hadnot Point before, the same pattern is occurring now also.

DR. ARAL: Okay.

DR. GOVINDARAJU: Because otherwise right now the analysis the way it's doing is not being influenced by what is happening at Hadnot Point. We're assuming that whatever concentration behavior we are observing, that is capturing everything. So that relationship changed over time, then it's going --

DR. ARAL: The answer is in this matrix. Once you calibrate the groundwater flow model and calibrate your contaminant transport

model, you get your matrix system like this. Do you change that?

DR. DOUGHERTY: Yes.

DR. ARAL: How?

DR. DOUGHERTY: Because S depends on Q which depends on the pressure which is time-dependent.

DR. ARAL: It depends on q.

DR. DOUGHERTY: Little q meaning specific discharge. Sorry, I want to make sure I get it right.

DR. ARAL: But that happens to be in our system already in the matrix A, but the overall system that you have here, are you going to change aquifer parameters? Are you going to change the foundation coefficients? Are you going to -- you know, all of that is in there.

DR. DOUGHERTY: So it's a big linearization step to get from A to B.

DR. ARAL: My model is as linear as this one.

DR. HILL: It's not only a linearization step, it's a very strong lumping step. You're putting a lot in there. What that produces is

a system that can't be cross-checked.

DR. DOUGHERTY: Well, there's nothing else to cross-check because he's using all the data.

DR. HILL: Yeah, you can't cross-check anything. You can't cross-check whether the hydraulic conductivities make sense. You can't cross-check whether the source strength makes sense. You can't cross-check anything. And also, the data you put in there, all the fits you showed, fit the data points perfectly, which always makes me nervous. So how do you deal with data noise as well?

DR. ARAL: First of all, cross-checking hydraulic conductors, it doesn't interest me in this case because I'm not using this differential equation to generate matrix A. I'm not using this differential equation to generate the matrix M or S. That's irrelevant. I really am looking at ten observation points characteristics for their behavior based on a database.

Now how am I going to propagate the error that I have in those observation points? The bands that I have described earlier is

going to give us information. If we have field data error it will propagate in our solution. We will have computational error. It will propagate in our solution.

DR. DOUGHERTY: Even though your interests may not lie in matching conductivity values, the consistency between a data-driven system and a physics-based system are going to provide some measure of comfort to a lot of people.

So one possibility that might be considered is to take local scale flow and transport models, and so your original differential equation system, apply it to a measurement matrix so you basically are condensing the system down to the number of monitoring locations. And then comparing the condensed matrix coefficients to the coefficients that are derived out of this linear control system.

And I understand, I understand, but because you've got, they aren't going to be the same because to get to a linear control system you have to do, you do have to do some linearization. It's true, but it may help

with some comfort to look at those, to look at a static condensation of the finite element matrix, you want to think of it that way, versus a control matrix.

DR. ARAL: The way you come up with the matrix A in a finite difference or a finite element method is completely different.

DR. DOUGHERTY: I understand.

DR. ARAL: But you should also ask the question to the person who's doing or choosing that path to give the comfort level of predicting the assimilated or observed values, right? And that's what you do. That's what you do. And in this case that's what we have done. We have totally used a different method to generate the matrix A or B, and we have confirmed the outcome that we have observed at the site are a match.

DR. CLARK: Richard is the next one in line, and [then -ed.] we're going to have to move on again I think. This is something that we may want to come back to if we have time this afternoon.

But go ahead.

DR. CLAPP: Yeah, this actually might be a

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question that jumps the gun. I'm actually wondering about at the bottom of the, at the end of this process how does this advance identifying finished water at a location where a child with a birth defect lived? What their consequence was or at least what their categorization was.

DR. ARAL: We have discussed that partially. We can use this method to determine the concentrations at water supply wells as a profile as well if we have information on concentrations. So once we have generated our profiles as solution, for example, if this is our water supply well data, if we are predicting this, our predictions will be used after this point the same way the other procedures would have used it going through groundwater flow, contaminant transport modeling.

DR. BAIR: It's a follow up. So if you do this at those three locations that are the local locations you indicated on the one map where the spots came out?

DR. ARAL: Any. Any location. Not three.

DR. BAIR: I thought you said you were using

at the three where you had the most data and it couldn't be applied at areas --

DR. ARAL: We have not, we have not decided where we will use this yet. We are going to be totally data driven in that aspect. I am just giving you here some characteristic small locations that we may use.

DR. BAIR: Okay, so you could take that gold spot and move it all the way out along the line of wells that extends to the west where there's not much data at all?

DR. ARAL: The answer to that question is here. If there is no data, we will not use this.

DR. BAIR: Okay, so there will be water supply wells in the area we've talked about today where you can't apply this method.

DR. ARAL: Right. If that is the case --

DR. BAIR: So then what is used for the exposure assessment if this method doesn't apply? You still need a deterministic flow and transport model?

DR. ARAL: That's a good point. If we don't have, if there are water supply wells around here which we are using to contribute to the

whole system supply or add to the system supply, then using water supply concentration profiles here is not going to add as much information for the whole picture.

DR. BAIR: So my question was how many water supply wells will be left out?

DR. ARAL: I have not looked into that yet. I don't know what the data structure is. We are just working on the method.

DR. BAIR: So it does mean that there will be two approaches to the same problem running in parallel?

DR. ARAL: Uh-huh.

DR. BAIR: Is that right?

DR. ARAL: That's correct.

DR. CLARK: Why don't we move on. Morris.

MR. MASLIA: I may not have shown it, but somewhere in the notebook there was a flowchart, and it gave a double path. One was the traditional fate transport model, whether we use deterministic, probabilistic or grabber* estimation. The other approach was using this screening level model, and that would, depending on the data that you have

available, would determine the approach.

STRATEGIES FOR RECONSTRUCTING CONCENTRATIONS: PRESENTATIONS AND PANEL DISCUSSION NUMERICAL METHODS

At this point I think we're going back to the traditional method that we had a lot of questions about this morning, but then the purpose of this is to at least generate some alternatives or get more input from you. So Rene Suarez started halfway as we completed the Tarawa Terrace modeling or as part of that, and we'll move into Rene's presentation.

MR. SUAREZ: Good afternoon. My name is Rene Suarez as Morris said. I am with ATSDR on the Exposure Dose Reconstruction Team and during the next few minutes I will be talking about the proposed approach to numerical groundwater flow and contaminant fate and transport modeling for the Hadnot Point and Holcomb Boulevard study.

The outline of this approach and kind of this presentation is groundwater flow modeling on the regional scale. Here we are going to develop and ^ [calibrate ed.] a steady-state model. We as well ~~we~~ [-ed.]are going to develop and calibrate a transient model for the groundwater flow. Then we will

have to develop and calibrate groundwater flows for the local scale where we have the contaminants ~~of~~ [in -ed.] the areas of concern. And ^ [calibrate -ed.] contaminant fate and transport models for those ~~local~~ ^ [locally refined -ed.] models.

First of all I'll describe a little the Tarawa Terrace model. I know some of you were involved in the expert panel on this. The approach is very similar so I will just briefly describe the approach that was used for Tarawa Terrace.

In the yellow box we have Tarawa Terrace and what was used there was~~,~~ [-- -ed} we developed and calibrated a groundwater flow model in MODFLOW. It was a steady-state model. Then a transient model was developed. From that we developed and calibrated a contaminant fate and transport model using MT3DMS, which gave us the concentration over time for the area of the model.

Then we used a simple mixing model to estimate the exposure concentration using the flow data of the supply wells and the concentrations from the model. And finally,

we verified ~~those~~ [the -ed.] estimated exposure concentrations in ~~that~~ [the -ed.] water distribution model that was ~~building~~ [built in -ed.] EPANET.

In this slide I'm showing the proposed Hadnot Point/Holcomb Boulevard model. And first I would like to point out the difference in areas of the Tarawa Terrace model that we have here in the yellow box and Hadnot Point and Holcomb Boulevard.

The area is five square miles for Tarawa Terrace, and I think Morris in one of the slides had 50, but the proposed [area is 84 square miles -ed.], I think that was like [, ed.] this is a more updated area. It's about 17 times larger for this model. The size of the total domain is 51,000 feet in the Y direction and 45,000 feet in the horizontal direction.

Some of the features of this model we have [are -ed.] a specified head in ~~data~~ [layer -ed.] number one of this model. That is representing New River here in this dark blue. On the right side, or the west side of this model, we have a no-flow boundary that

mostly represents a topographic divide.

MR. MASLIA: Excuse me, Rene, can you speak up a little?

MR. SUAREZ: Yeah, sure.

We have a no-flow boundary on the ~~west~~ [east -ed.] side [which -ed.] is represented by a topographic divide. In some areas we have some general head boundaries where we have supply wells. We also have about eight small creeks that are represented by drains here in the model in green, and we have 100 supply wells in the area of Hadnot Point/Holcomb Boulevard.

In terms of the grid design that we are proposing, the model has been subdivided into 343 rows, 303 columns. This gave us square cells of about 150 feet per side. The model had been subdivided vertically into ten layers.

On the right side of this slide we have a table where we have the geohydrologic units on the left-hand side and the corresponding model layers on the right side. We have seven aquifers and seven confining units. The confining units are underlined in

red. And please notice that the Brewster Boulevard is lumped into one model layer.

Horizontal hydraulic conductivity for the different aquifer was obtained from aquifer test analysis [. -ed.] ~~for~~ [For -ed.] the confining units~~.~~ [-ed.] ~~It~~ [it -ed.] was assigned a constant value of one ~~fit~~ [feet -ed.] per day. Effective ^ [recharge or infiltration -ed.] was obtained from precipitation data, kind of the same approach that Bob described earlier that was used in Tarawa Terrace.

And elevation of the different layers, elevation for the, for layer one, the top layer, was obtained from ^ [digital -ed.] elevation model [and -ed.] topographic information and ~~for~~ [-ed.] the elevation for the other layers was obtained from borehole log data and geophysical data.

From here we proceeded to -- and please understand. This is the proposed approach, so it's not really like in the step of being calibrated or being completely built. So just keep that in mind while you're thinking there.

So the model was calibrated using ~~that~~ [kind of a -ed.] trial and error approach first~~, kind of a code approach~~ [-ed.]. And then the PEST optimization is going to be or was run under this model, this steady-state model. Over here in the center we have horizontal hydraulic conductivity.

The layers that are currently missing are the confining units that were not included in the PEST optimization at this step. ~~Research [? -ed.], two ^ [parameters -ed.]~~ [Two recharge zones -ed.] were identified during the calibration process~~,~~ [. -ed.] ~~and~~ [And -ed.] basically what we're doing is trying to review this ~~subjective~~ [objective -ed.] function in the PEST optimization. The objective function is just the sum of squared error. This is the observed heads, and this is the simulated heads. This simulation~~, ~~[– ed.]the PEST optimization~~, ~~[– ed.]took 78 MODFLOW simulations, and it took about two hours to perform that.

MR. HARDING: Can I ask you a question?

MR. SUAREZ: Sure, sure.

MR. HARDING: I guess I'm not a groundwater

modeler. Why are you calibrating the recharge when you can make a reasonably good estimate of it and it's a time series?

MR. SUAREZ: Well, we're going to use both like we have in some starting points some precipitation data, weather data, but we still don't have, we only have like one weather station for that whole area and recharge definitely should vary in that area. So it's still going to be a parameter that we want to include in the calibration process.

MR. HARDING: You could get gridded precip.

MR. SUAREZ: You can get what, sir?

MR. HARDING: You can get gridded temperature and precip from the PRISM database on a four-kilometer grid, which is not super fine, but it's better than your weather station probably. Anyway, I disagree.

DR. DOUGHERTY: This is the net of what actually gets in the ground.

MR. HARDING: Yeah, you'd have to make that calculation, but you've got all the data to do it.

DR. HILL: But you don't. It's not something --

MR. HARDING: No, you don't.

DR. DOUGHERTY: Changes in soil moisture.

DR. BAIR: On a monthly basis, how much does that -- is that a problem? It's a pretty well drained area.

MR. FAYE: The only thing you've got are regional estimates of Blaney-Criddle stuff. You don't really have anything that you can pinpoint down to an area like this.

MR. HARDING: It's a starting point. That's where you start, but --

DR. DOUGHERTY: You've got the precipitation. These are pretty good estimates. They're interpolated from point [data -ed.]^. You've got temperature and dew point, you can use that in a physical-based equation to calculate ET. So then what am I missing about the rest of it? If the rain falls on the ground, where does it go?

DR. BAIR: Some's into ET, some's into plants, some's into runoff and some continues downward into groundwater.

DR. DOUGHERTY: And some stays in storage.

DR. BAIR: And some stays in storage until something happens to it, maybe in your 18

model.

MR. HARDING: Stays in storage in the surficial layers?

MR. FAYE: In the soil moistures.

MR. HARDING: Doesn't it make sense to use this information to inform this somehow? Because, I mean --

DR. DOUGHERTY: Usually something like that would be a starting point. You get a rough number and use a starting point.

MR. HARDING: Rather than just calibrating it. It seems to me you know a lot about it from the precipitation --

DR. HILL: So you'd expect it to be that value maybe, plus or minus a factor of maybe up to two, probably not more than two.

MR. HARDING: I'd be surprised if it was anything close to there.

Okay, go on, I'm sorry.

DR. BAIR: Rene, I have a question. Can you go back one slide?

MR. SUAREZ: Sure.

DR. BAIR: So if you look at iteration six, those are your best fit, right, the row going across from iteration six?

MR. SUAREZ: Yeah, well, I will call this it was the best fit without considering any specific information about the different layers and that, but, yeah.

DR. BAIR: So then if you look at model layer four, that's an aquifer.

MR. SUAREZ: Uh-huh.

DR. BAIR: And model layer three is a confining layer and five is a confining layer?

DR. DOUGHERTY: No, no, he said he didn't include any confining --

DR. HILL: He said estimated --

DR. BAIR: No, no, they're there. They're there in the model. Right, so my question is if model layer three has a hydraulic conductivity of one, and model layer four has a hydraulic conductivity of 1.2, and model layer five has a hydraulic conductivity of one, who's confining whom?

MR. SUAREZ: Well, these values were not really bounded like very specifically during the optimization process. That's why I'm presenting the approach. If we go to the green row, these values are more based on the aquifer test data. So, yeah, I expect these

values to be higher during the optimization process.

DR. BAIR: And I apologize. It's just hard for me as a member of the panel to tell what's final and what's preliminary, so if I ask too many questions it's because my impression is this is the final stuff that you're presenting and not some preliminary work.

MR. MASLIA: Now, let me just again clarify. I tried to find a nice fit between giving enough information so we could provide the methodology that we want to use and not committing too many resources that we've gone down the path of trying to calibrate a model and then receiving feedback from the panel that's not going to work or you need to make some major changes because then in terms of resources and efforts we need to back track.

I didn't want to not show or present anything so again, especially on the numerical modeling part more so than the data analysis because it's really --

DR. CLARK: I think they're going to be depending on you to recommend --

MR. MASLIA: -- just an approach.

DR. CLARK: -- forward.

DR. HILL: Can I make one comment on this? Just that when, in regression when you have parameters that go to unreasonable values, generally that's indicating that there's some conceptual problem with the model. So instead of just putting limits on that to keep it reasonable, I would suggest re-evaluating your conceptual model.

MR. SUAREZ: Sure, sure.

DR. KONIKOW: Well, another related issue is why not, if you want to assume all the confining layers have the same hydraulic conductivity, why not at least treat it as one parameter? Then why not estimate that? Just make it part of the whole system.

Well, on a conceptual basis maybe this is a good time to discuss it, but maybe go back to the previous slide. And one of my major conceptual concerns is for the flow and transport model lumping those four upper units into one model layer. This seems like a major conceptual flaw.

Somewhere in your report it said that you had field evidence that that upper clay

unit was very substantial in retarding the movement of the DNAPLs and had a significant effect on the contaminant ~~transporting~~ [transport -ed.], yet here you're lumping two aquifers and two confining units into one model layer, which means you're going to smooth out all the influence of the heterogeneity, and a very significant heterogeneity, in layering on contaminant transport.

And this is the unit into which the contaminants are introduced and you're losing all the controls by this lumping. I just don't see conceptually how this can be justified.

MR. SUAREZ: Well, one of the plans is to subdivide that when we go to the more localized model because this is --

DR. KONIKOW: Well, you -- I don't think when you go to the localized -- if you're using MODFLOW, maybe Mary could say something about this. I don't think in the localized models you could change the vertical, the model layering, can you?

DR. HILL: Yeah, you can.

Are you doing this to avoid dry cells?

MR. SUAREZ: Yes.

DR. HILL: Yeah, don't.

MR. SUAREZ: Well, it's one of the reasons -- let me explain. We don't have to the extent that we're proposing this model the, basically the interpolation scheme that we're using to interpolate those layers. Now you get a lot of layers that kind of like kind of disappear, appear and disappear, and it's kind of difficult to at this moment I'm not presenting at this moment just to have a structure that makes sense.

DR. HILL: Use the Huff* [HUF (hydrologic unit flow) -ed.] package and assigned, and use defined thickness layers using your contoured water table for those layers. And get in the ballpark in terms of hydraulic conductivity.

DR. CLARK: Rao had a comment he would like to make and then I think we need to let Rene continue his presentation.

DR. GOVINDARAJU: This is Rao from Purdue. I think along the same lines my feeling is even if you get the conceptual model correctly, and you just let the optimization

run its course, it may give disparate value the confining layers which are less than the aquifer conductivities.

I think once you think a conceptual model is correct, you must do a constraint optimization. If the assumption or the belief is that the confining layers are about one-tenth of the conductivity of the main layers, then you should, I suppose, impart that knowledge to the optimization routine.

DR. KONIKOW: But is that knowledge or is that just an assumption?

DR. GOVINDARAJU: That's an assumption.

DR. HILL: Well, I would say it's knowledge. It just depends on how you want to use that knowledge. And one way to use it is to apply it as constraints so that you constrain what values your parameters can take. Another way to use that knowledge is to say, okay, I'm not going to apply this as a constraint. I'm going to see what fits my data best and if those values are unreasonable, I'm going to sit back and say, okay, if I have enough sensitivity, if I have enough, if my targets or observations --

DR. CLARK: Let's let Rao go on and, ~~I mean~~ [then -ed.], let's let Rene go on and present his --
DR. HILL: I was almost done.
DR. CLARK: Okay.
DR. HILL: -- then go ahead and if my observations provide enough information to estimate those things, and they provide a lot of information, if my estimated value is wrong, it implies a problem with the conceptual model. So it's just how you use that information.
DR. CLARK: Let's let Rene go on and finish his presentation.
MR. SUAREZ: I will point out something maybe related to that. So just to show [how -ed.] the calibration was from that preliminary model as we mentioned we were using, we used PEST. One of the things we also are considering [is -ed.] UCODE. The root mean square for this model was 5.46, and on the right side we have a plot of the simulated versus observed water level values. The values in red are monitor well data, and the values in blue are supply well data.

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And please notice [in -ed.] this slide, overestimation of the supply well data because this was just to kind of like try the method. Because this includes all the data, one thing that when you go and check on case-by-case of the observed data, some of the observed data that I include I shouldn't have included in because it was being subjected to draw-down effect, and at this time we're not concerned with pumping. So there's a lot of refinement that I have to go and select what data I will include into the optimization process.

DR. DOUGHERTY: Quick question, and all these are equal weights?

MR. SUAREZ: What?

DR. DOUGHERTY: You're using equal weights on all of the data?

MR. SUAREZ: Yes, right now, yes.

DR. DOUGHERTY: So you're not using the measurement error differences?

MR. SUAREZ: No, at this moment, no.

So this just showed the results from that preliminary model, and we have a head difference of about four feet from east to

west. This plot also showed the head residuals. We have in blue less than minus five feet, in green minus five feet to five feet, and in red, larger than five feet. One of the ~~common~~ ^ [comments -ed.] about data density that ~~we're~~ [we were ed.] talking before, although this model is really large, actually the area is very concentrated, and it's hardly difficult to calibrate the models in some areas that we don't have data, and at this step we're just trying to build a regional model and then we'll have to calibrate that model. But then we'll have [, ed.] I will say[, -ed.] plenty of data to calibrate those local models.

Just comparing the Hadnot Point/Holcomb Boulevard and the Tarawa Terrace model side-by-side I just want to point out what I would think is the two major difference in terms of building these two models. We have fairly [large -ed.] difference in [the -ed.] size of the model. That will include steps that were not contemplated, were not in Tarawa Terrace. Like here we will have to build a regional model and go to more refined

local models.

Also, we have a lot more data that is good for calibration, but it will also make it more complex. So we will need to ~~do,~~ [-ed.] use optimization process for this model. And that will include a lot of effort in calibrating the steady state transient models for each one of the regional/local models and the contaminant fate and transport.

DR. HILL: Excuse me. Those observed the concentrations that you have listed there, do they include the non-detects?

MR. SUAREZ: No, these are locations. If you look at this I may not have made the difference. Locations where we have data in terms of contaminant --

DR. HILL: It is important to use the non-detects as well, and UCODE provides a formal mechanism for using non-detects.

MR. SUAREZ: Sure, sure. I saw that in your notes. And definitely that's something that we'll contemplate.

So we can proceed with the discussion. What I want to do is summarize ~~like~~ [-ed.] the approach, so you can see in perspective of the

amount of data that we have at this moment and amount of data that we may need to check within the documents that we still haven't really realized that we have.

We are going to build our numerical model, and we gave some information of a preliminary numerical model that we have built. We are going to run a steady state model. We also gave some preliminary information on that. We are going to run this model using MODFLOW-2000 and PEST for calibration. We're going to do that as well with the transient model, same situation. Then that's for the regional model.

From there we're going to go to a more localized model where we're going to choose some areas where we need refinement. And when I said refinement or local areas, the bulk of our contamination is located, for example, in this picture, the landfill area and the HPIA area, Site 88, we'll need to build local models for them.

We will have to evaluate the effects of pumping on those because we have a lot of supply wells and not all of them are pumping

on the same times. So we'll have to evaluate the effect of pumping on those boundaries. And from there we'll have to run our transport models in those local grid refined models ~~or ^ models~~ [-ed.] using MT3DMS, the same approach that was used in Tarawa Terrace and PEST or UCODE for calibration.

From here we can start the discussion.

DR. BAIR: Rene, with respect to the calibration, is there any time, money -- they're kind of both the same anymore -- to get a velocity data that you could use to help calibrate? You have a lot of head data, but it would be nice to get, and I know it's not easy here, stream flow gain or loss so you can get some discharge data, a flux out of your system. Or some tritium/helium age dates so you can do some backward particle tracking to check to see if the physics of your model matches the chemistry of the tritium/helium to give you confidence in some of the velocities.

MR. SUAREZ: I'm sorry, you're combining something about money or I was just thinking --

DR. BAIR: No, the money was just a comment

for the people way up there. That's for the people in the corner. You're on a time frame and time costs money and this would be getting more field data. So can you put in a couple monitoring wells out in that area where you don't have a lot of data?

MR. MASLIA: Let me address that specifically because that's what I picked up on the field data. Can we gather more field information, which we could gather in a shorter span of time compared to the effort of doing a full-blown calibration here. And that would really depend on discussions from our agency management and the Navy or the funding party. And could it either meet our existing time schedule or extend it less longer in time.

And that was one of -- I'm glad you asked that question because it fits right into, and maybe it was not clear why we went to Dr. Aral and his group at Georgia Tech to try to come up with an alternative method. After we finished Tarawa Terrace we saw the effort that went into it. And regardless of if you think the confidence is not large

enough or narrow enough, you have a model that produces reasonable results.

And we saw the effort that went into it. Looking at what we had, just looking at the data that we have, it became apparent right away is what can we do to come up with some initial answers, not throwing out the baby with the baby carriage at the same time, but either using it as a starting point to help augment or help us jump start that or as a check.

As somebody said if we're going to spend another year or two years, you still have the question of how confident are you in those hydraulic conductivities or how confident are you in a much, much larger model. And so I made the decision to see if we could come up at least with a screening-level model, you know, something to put our teeth in.

I think your suggestion we need to talk about and think about could that Dr. Aral's method then also be combined in conjunction with maybe a small field effort to give us a method and some information to more

rapidly get to the point of where we now want to distribute the --

DR. BAIR: I mean, I guess what I was getting at, Morris, is there a couple obvious areas where you need data? In the north part of your model area where you don't have many water levels, there aren't many pumping wells up there so a current water level would actually give you some guidance for applying backwards in time.

I also think you need to look at some of the confining layers in more detail, not only their lateral continuity but their permeability because they're restricting the contaminants flowing downward. And assuming one foot when the aquifers are ten feet per day, you know, a difference of a factor of ten isn't much of a confining layer. It's just the heterogeneity within most aquifers.

So I just thought it would be your time, Rene -- and I didn't mean to scare you with that and somebody else's money, but I just thought if there's an opportunity to discuss that, that there are some -- I don't think it's expensive. It's time that I got

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the impression that's pushing you.

And I personally would much rather you see take the extra year to get the answer right or closer. And it reminds me of that Jack Nicholson film with Tom Cruise where they were in the Marines and there was that -- what was the name of the movie? A Few Good Men, yeah.

And I show that, a clip in my class, and Cruise is on the stand and Nicholson says, "You can't handle the truth." Well, I turn that around and say, "You can't afford the truth." How much of the truth do you want to pay? And in the bottom line when you're done would have spending 25,000, 50,000, 100,000 more dollars to get more of the truth and lose a year, is that going to be beneficial. And that's not a decision for the panel. That's a decision up there. So that's my two bits.

MR. FAYE: Dr. Bair, how much differentiation in time can you get from the age-dating analyses that you're talking about? What was it, a helium/tritium type?

DR. BAIR: Well, I use this with one of my Ph.D. Students up at Woburn, and we used the

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tritium/helium dates to help calibrate our flow model. So we, too, were forecasting backwards in time, and what we were interested in is if our steady-state model or our transient model prior to turning on the wells, wells G and H.

Now that the wells were off in 2002, when we did the sampling, could we replicate those velocities in our model that we measured in terms of the groundwater ages in 2002. So they're two different times, but neither of them are transient at that moment because neither of the wells were on. And that gave us a comparison of physics-based travel times and chemical-based travel times. And it turned out to make us feel comfortable.

So I think what everybody's looking for here is for your models to demonstrate a level of professional comfort among all the different professionals in the whole room. And if tritium/helium helps you or some other technique helps you --

MR. FAYE: But what is your tolerance on those ages? I mean, is it like of you get an age of 1950, does that mean it was somewhere

between 1940 and 1960 or, I mean, what's the tolerance there on that?

DR. BAIR: I have my Woburn presentation in here. Kip Solomon* did those for us at the University of Utah, and he puts an error bar on every one of those. So the error bars there are less than a year, slightly more than a year. And then we compared it to the error bars on our reverse particle tracking, which accumulates a conservative age.

And our error bars there were putting particles all over the well screens and tracking them backwards to the water tables. So we were looking for our variation in backwards travel times to be within Kip's plus or minus. And we did it pretty well except for the deepest wells that were closest to the metamorphic bedrock where they get a helium signature from the decay of some of the minerals in the granite.

So that's esoteric, but I think you need a little more field work.

DR. CLAPP: I was just going to ask Dr. Bair, actually, my impression is that that additional work in Woburn hasn't changed the

results of the case-control study. And in terms of how it's implied or applied in epidemiologic study it may be been --

DR. BAIR: It's done subsequent to the case-control.

DR. CLAPP: Right, I understand, but would it have mattered in terms of the case-control study as an outcome?

DR. BAIR: I've shown our results to the Massachusetts Department of Health people, and they wished, they told me they wished they had had this when they had done their work. What my student was able to do is what you're asking yourselves to do is to come up with a month-by-month exposure concentration for each one of the water districts in Woburn.

Woburn has a very mixed system so the water distribution model was much different. And we're able to come up with bands of what the concentration would have been during gestation, during the first year, seven years, et cetera. And they didn't have that. I don't think most epidemiologists are used to getting that type of information. So it's something groundwater people haven't been able

to provide with much confidence until the last many years. But, no, it didn't change them. They had already published it so Costace* and Condon*...

UNIDENTIFIED SPEAKER: (Inaudible).

DR. BAIR: I don't know. They would have had to have different approach because I, we can give exposures. I don't know if in terms of parts per million, micrograms per liter.

DR. CLAPP: They were looking at ranks and I doubt that the ranks would have changed much to be honest.

DR. WARTENBERG: Why didn't they re-do it if your data were available?

DR. BAIR: What's that?

DR. WARTENBERG: Why didn't they re-do it, their analysis?

DR. BAIR: I don't know, budgets.

MR. BOVE: I'll tell you one thing, if they have all the data it can't cost that much.

DR. BAIR: One of the problems we had there was statistics of really small populations so there are 28 children who developed leukemia in Woburn over that period of time, '68 to '84. Seven of them were involved in a

lawsuit.

It's the lawsuit testimony that gave us the birth dates and the gestation periods. The other 21 sets of data are sealed by the State of Massachusetts under a nondisclosure agreement. So I have seven. I wish, you know, I tried bribery. I tried lunches, tickets to the Ohio State-Michigan game, everything and couldn't get those released.

MR. FAYE: Dr. Bair, let me ask another question. Most of the wells that were contaminated are destroyed now. They're not available for sampling, so what would an alternative be if we're lucky enough to have like a monitor well along the flow path or --

DR. BAIR: Yeah, you would want to use monitor wells along a flow path, and that's what we used more as a pre-pumping wells, G and H, potentiometric surface and particle tracking for was to determine a long flow path and then sample wells at distance along that flow path and then at depth.

DR. CLARK: Morris had a question.

MR. MASLIA: Yeah, a question. Combining two thoughts here, wells G and H at Woburn,

I'm thinking they may, assuming you've got the data, there may be an opportune moment here to test out Dr. Aral's method on some real data.

DR. KONIKOW: I have a couple things, but one, you know, I think there can be some value to doing age dating, but I do think you have to be careful. This system has been so heavily pumped. Things have been mixed up so much in this system.

You have boreholes that are open to multi-aquifers. You have flow down the annulus. Getting an undisturbed, natural, a sample that reflects an actual travel time through the system under natural conditions. It may be difficult. It may be impossible. I don't know. I'm not saying don't do it. I think there is value of getting those age dates. But the band of uncertainty about your ages may be wider than the geochemists will tell you on the basis of the lab analyses.

Another point if we jump to the transport modeling -- well, let me go back one step. Again, on the age, the point I was trying to make there, whether or not you do the age dating and get the samples, I want to

follow up on something that Scott suggested and reinforce that the use of MODPATH to simulate advective transport.

Even though it doesn't give you concentrations, can give you for such a low computational effort and low computational cost a lot of insight into how fast things are moving, where they're going, what the effects of transient flow are. Extremely valuable to improve your conceptual understanding at almost no cost. I mean, this is really relatively easy to do once you've developed a reasonably good transient flow model. And it's just a logical step to do before you go to the, all the headaches of transport modeling. And so I would really encourage you to add a few days or a few weeks to the timeline to get a lot of insight from the MODPATH.

MR. MASLIA: That's what we added. People would love it.

DR. CLARK: Mary and then Walter and then we need to get back on our video streaming again.

DR. HILL: Two things. One is you also mentioned stream flow data, and Cudgels'

[Codgels -ed.] Creek -- I don't know if I'm pronouncing that correctly -- is entirely within the model and there's, actually, you have several streams that are entirely within the model and many of them go under roads which provides perhaps when the road was constructed, they might have done some kind of analysis about stream flow that you can use to get a low flow measurement. You might have a fairly large, a small weight, a large variance on that. But it's extremely important to have some kind of flow data to compare your model against.

MR. FAYE: The USGS in North Carolina does have their standard regression equations with soils and drainage area and whatever for estimating average flow conditions and things like that. Probably in the upstream reaches of these streams that would be a possibility. The downstream reaches are all tidally affected, and Wallace Creek is tidally affected big time. So we could definitely take some shots at estimating a long-term average, low flow or average flow, whatever.

DR. CLARK: Walter, go ahead.

DR. GRAYMAN: Just briefly, just actually going back to what Ben was saying. I wasn't quite satisfied with the closure on the recharge issue. Within PEST do you set bounds on the, do you give it an initial recharge value and then set bounds on it and allow it to --

MR. SUAREZ: Yes, an initial value and you can set your bounds --

DR. GRAYMAN: I think we may be getting a little bit into an interface issue. And I'm talking about here an interface issue in terms of professions between surface water hydrologists and groundwater hydrologists. And then I think Ben is probably the only one here who's probably kind of the official surface water hydrologist.

MR. HARDING: ^.

DR. GRAYMAN: Well, but we're all hydrologists. I'm not sure that we really explored that as much as possible because I tend to agree with Ben. At least surface water hydrologists feel they can fairly well accurately estimate what the amount of water, at least entering the upper zones of the soil

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than maybe what groundwater hydrologists feel surface water hydrologists can do. I'll leave it at that.

DR. CLARK: Let's wrap it up then. We have, it's our break time, and we reconvene at 3:30 at which time we'll hear questions from the public.

(Whereupon, a break was taken between 3:15 p.m. and 3:30 p.m.)

MR. MASLIA: Panel members here because there's a decision or a thumbs up or thumbs down approach for the panel to -- because it's really your decision as panel members. So I'll just wait 'til all our panel members are here.

According to the schedule, we're supposed to have another half hour of discussion and then go into the public presentation part. We have allotted two hours. Right now there's a 30-minute presentation by a member of the CAP, Jerry, as well as a presentation-slash-statement by a member of the Department of the Navy, Dr. Dan Waddill.

What we're proposing was brought to my

attention by Scott Bair is he's got a prepared presentation for other purposes about Woburn that may have some important information for us in terms of what we're doing here at Camp Lejeune and I would be interested in it from a professional standpoint if nothing else, and it may, in fact, generate more questions.

So what I'm proposing is that we move the public presentation to start now. Do the public presentations and then we should have sufficient time for Scott to make his presentation and then we can follow that with additional questions. Is there any issue? Does anybody on the panel have an issue with that adjustment to the schedule?

Walter?

DR. GRAYMAN: Can we move Scott's to right at the end, the last thing?

MR. MASLIA: That's after the public presentations.

DR. GRAYMAN: Okay so the stuff you were talking about --

MR. MASLIA: Well, no, not his but it may add more information that we want to take into account to, and so we would basically end the

day with maybe a longer discussion period than that. So is there any, is that okay with everybody?

DR. CLARK: Is that a problem with the, Dr. Waddill and Mr. Ensminger?

MR. ENSMINGER: No.

MR. MASLIA: So if that's the case we're into public presentations.

PANEL CHAIR ACCEPTS STATEMENTS AND QUESTIONS FROM PUBLIC
(REPEAT STATEMENT OF PURPOSE OF PANEL)

DR. CLARK: According to protocol I'm supposed to read the charge again to the panel so that everybody will know that this is a public meeting and what it's supposed to accomplish. So in order to follow protocol I'm going to do that if you'll bear with me.

This is an expert panel assessing ATSDR's methods and analysis for historical reconstruction of groundwater resources and distribution of drinking water at Hadnot Point, Holcomb Boulevard and vicinity, U.S. Marine Corps Base, Camp Lejeune, North Carolina. The purpose and scope of this expert panel is to assess ATSDR's efforts to model groundwater and water distribution systems at the U.S. Marine Corps Base, Camp

Lejeune, North Carolina.

This work includes data discovery, collection and analysis as well as water modeling activities. To assist the panel members with their assessment, they have been provided with the methods used and the results obtained from ATSDR's previous modeling efforts at Camp Lejeune which focus on the area of Tarawa Terrace and vicinity. The panel is specifically charged with considering the appropriateness of ATSDR's approach, methods and time requirements related to water modeling activities.

It is important to understand that the water modeling activities for Hadnot Point, Holcomb Boulevard and vicinity are in the early stages of analysis; hence, the data interpretations and modeling methodology are subject to modifications partly based on input provided by members of this panel.

ATSDR expresses a commitment to weigh questions from the public and to respond to public comments and suggestions in a timely fashion. However, in order for this panel to complete its work, it must focus exclusively

on data discovery and analysis and water modeling issues. Therefore, the panel will only address questions or comments that pertain to data discovery and analysis and water modeling efforts.

For all non-modeling water questions or statements, the public can contact the ATSDR Camp Lejeune Information Hotline at telephone ~~7-7-0-4-8-8-3-5-1-0~~ [770-488-3510 - ed.] or e-mail atsdrcamplej@cdc.gov.

REPRESENTATIVE OF CAMP LEJEUNE COMMUNITY ASSISTANCE PANEL (CAP)

And with that, why, we can begin the public presentations and we're going to hear from Jerome Ensminger first.

MR. ENSMINGER: Good afternoon. My name is Jerry Ensminger. I am a member of the ATSDR's Camp Lejeune Community Assistance Panel, and I've been involved in this incident since August of 1997. Over these past 12 years I have viewed thousands of documents related to this situation and what I have discovered is both disheartening and disgusting.

Department of the Navy and United States Marine Corps officials and representatives have in the past and continue

right up to the present to misrepresent and deny the facts. They have done this by making false and misleading statements, providing incomplete or false data and by withholding key data that is crucial to the findings of truth in this situation.

I don't expect any one of you to take my word as proof of these serious allegations I'm making against these supposed honorable government entities. That's why I've provided all of you with some of the actual historical documents which came directly from their files so you can witness the deception with your own eyes.

Now, I want to take you through some of these documents, and you have them in a binder there in front of you, and I've picked out some key documents. And these are only a few examples of what went on here.

But the first document is a letter dated 3 February from 1986 from the United States Environmental Protection Agency Region Four. And it states, "Dear Sir: On November 1st, 1985, Messrs. Mathis and Holdaway of this Agency met with Facilities Engineering Staff

at Marine Corps Base Camp Le Jeune."

Okay, I want to skip down to the second paragraph, what's highlighted on your document. "Both Messrs. Holdaway and Mathis became aware that there was evidence from sampling as early as 1983 or 1984 of diffuse contamination of the groundwater with unspecified organic substances, and that as a result of detection of unspecified volatile organic compounds in raw potable water samples, certain potable wells at Hadnot Point were taken out of service. In consideration of the fact that the major portion of the resident population of Camp Le Jeune is dependent on Hadnot Point well field as its potable water supply, the parties in the meeting agreed that any potential contamination of this resource should be investigated as expeditiously as practical. It was also established that there was no contamination detected in treated potable water..."

Let me say that again. "It was also established that there was no contamination detected in treated potable water distributed

at Camp Le Jeune, however the extent and sensitivity of analytical procedures for specific organic substances was not fully discussed."

This was 1986. They found contamination in the potable water at the tap in Camp Lejeune as early as 1980. Let's go down to the second page of that letter.

It says, "This Agency is concerned that a potential for human exposure to hazardous substances and hazardous wastes via the Camp Le Jeune water supply may exist due to the presence of such materials in the groundwater in the general vicinity of the potable well field. The existence of such a potential exposure would warrant consideration of this area for inclusion on the National Priority List, with an attendant increase in the expediency of investigation and remediation." Now, the EPA didn't believe them and that's why they recommended this to go on.

Now, this next document comes from a technical working committee which was the predecessor to the Restoration Advisory Boards

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for the EPA. And they had members from the EPA. They had members from the state environmental regulatory agency there. They had members from the local community there. They had members from the ~~LANDIV*~~ [LANTDIV – ed.]. And this is a court-recorded document, and the gentleman by the name of Bittner was the City Manager for Jacksonville. And they were discussing the contamination in the Hadnot Point system at this point.

And Mr. Bittner asked the question, "What kind of tests were you getting when you were running those contaminated wells in terms of water quality?" He says, "I imagine it would be pretty much diluted but you were still probably getting some readings if you ever took a scan."

Mr. Bob Alexander who was the environmental engineer for Camp Lejeune answered his question. He said, "We had very little, if any data, before we realized our ground water was contaminated." I mean that is an out-and-out lie.

So Mr. Bittner follows up. "So there's no record of it in terms of what you

were pumping." Alexander, "We had some tests--like at the Tarawa Terrace area--before we realized that ABC Cleaners was polluting our wells there. We had some tests and ended up with some measurable concentrations. But they were almost at the detectable level. When you're taking out of the Hadnot Point area 35 wells that had been servicing that system, probably a well would only run for about two days. It would only be about five or six wells running, so we had a rotating cycle of operating on those wells. It would be practically impossible to say what wells contributed what compounds on any given day. You'd have to backtrack from the residence time in the reservoir and all that to see what wells were going two days ago."

So Bittner says, "And, basically, Bob, there's no record of that." And he says, "It would be practically impossible to track that down."

And then Ms. Cheryl Barnett, who was a representative from ~~LANDIV~~ [LANTDIV -ed.] up in Norfolk, Department of the Navy, who is by the way now a high ranking official up there

with their environmental branch, Barnett pipes in and says, "There were no requirements, you know, the requirements to test your finished water for VOCs; it's a new requirement. It's a new EPA drinking water requirement, so there was no prior testing program before. It is just purely in the course of this investigation that we discovered that problem to begin with and since that time they've been monitoring the finished water effluents, but it was never a requirement."

Now, that statement, "it was just purely in the course of this investigation that we discovered that problem to begin with..." This is a person that was trusted with our environmental health. She is a high-ranking official now in the Department of the Navy's environmental program. I want you take a look, and she was talking about the confirmation study when they discovered this contamination.

This letter was written on 10 August, 1982, by Grainger Analytical Laboratories out of Raleigh, North Carolina. The chemist up there and the part-owner of the laboratory saw

these samples, saw the interferences in the TTHM testing that they were doing, and they took it upon themselves to isolate the interfering chemicals and quantify them. And they wrote this letter to the Commanding General of Camp Lejeune.

Previously all samples from site TT and HP, which is Tarawa Terrace and Hadnot Point, "presented difficulties in performing the monthly Trihalomethane analyses. These appeared to be at high levels and hence more important from a health standpoint than the total Trihalomethane content. For these reasons we called the situation to the attention of Camp Lejuene personnel. Results: The identity of the contaminant in the well field represented by samples 206 and 207 was suspected to be Tetrachloroethylene.

And at Hadnot Point it was Trichloroethylene. If you'll go to the second page of that letter, there's where they broke it down. Those were the results that they got from those samples. Sample 120 was Hadnot Point tap water, 1,400 parts per billion.

Whenever the fuel leak took place at

the Holcomb Boulevard water system in January of 1985, they called the state in to do split samples because they thought they had all their contaminated wells offline already anyhow. Guess what? They still had one, one contaminated well online, Well 651 at Hadnot Point. They had shut the Holcomb Boulevard plant down and opened the valves up and put them back on Hadnot Point water to flush the system out, to flush the fuel that had leaked out of a backup generator line into their treated water storage tank.

These were the samples, these were the results of the samples that the state took. Now, this was dated, well, you can see the date of the analysis, February of '85. Now these people sat in these meetings subsequent to these tests, these analytical results and those initial letters that I read to you, and lied. I mean, this was one contaminated well that was creating these results in February of '85, 1,148.4 parts per billion at the elementary school in Berkeley Manor housing area.

If you'll go down to your next

document which is a TTHM test. When the TTHM regulation was coming into effect, the Department of the Navy contracted with the Department of the Army to have their environmental hygiene team come to Camp Lejeune and other Naval facilities and do, start doing TTHM tests for their water systems. You can see this one was dated 29 December, 1980. The first test that they did was in October of '80. You can see what they wrote down here at the bottom, heavy organic interference. You need to analyze for chlorinated organics by the GC/MS method.

Go to the next one, January of '81. You need to analyze for chlorinated organics by GC/MS. February of '81, water highly contaminated with other chlorinated hydrocarbons, in parentheses, solvents. Yet these people sit in meetings and say they didn't know?

ATSDR, you know, while they've had their own faults throughout this process, has had one devil of a time trying to get information from these people. There has been stonewalling, you name it. This is a letter

written on September 2nd, of 1994 from ATSDR to what was known as the Navy Environmental Health Center then, complaining about Camp Lejeune, about the Marine Corps and Department of the Navy, about getting documents and data.

ATSDR identifies and obtains documents needed for evaluation to develop the public health assessment by discussing the public health issues with the installation and having them send us documents where the information can be found. As you are aware, we have had much difficulty getting the needed documents from Marine Corps Base Camp Lejeune. We have sent Marine Corps Base Camp Lejeune several requests for information and, in most cases, the responses were inadequate and no supporting documentation was forwarded. That was September 2nd of 1994.

Go down to these e-mails. Ms. Kelly Dreyer, who worked at Headquarters Marine Corps, was put in charge of the Camp Lejeune water contamination issue. ATSDR had been provided incorrect water system data for not only the public health assessment, but for a study that was being done on small for

gestational age in adverse pregnancy outcomes. They never told ATSDR that the Holcomb Boulevard water system wasn't constructed until 1972.

ATSDR went through this entire process thinking that those, all those housing areas on the other side of Wallace Creek on the main part of the base, three major housing areas: Midway Park, Berkeley Manor and Paradise Point were always on that clean Holcomb Boulevard system. Well, the study period for ATSDR was 1968 through 1985. Well, the Holcomb Boulevard plant wasn't built 'til '72.

When I first saw that study, and it came out -- well, it came out a long time ago, but the first time I really looked at it in depth, I said what the devil's going on here. They only had 31 babies identified in that study as being long-term exposed in utero to trichloroethylene, TCE. I said that can't be right.

I called Dr. Bove up -- I didn't call him. I sent him an e-mail. And he sends me an e-mail back and he goes what the hell are you talking about. So I picked the phone up

and I called him, and I said you had I don't know how many thousand housing units over there, I said, that was, I said, the Hadnot Point water system wasn't constructed 'til '72. I said you only identified 31 babies in this study as being exposed to trichloroethylene, and I said, all those housing areas were on Hadnot Point water all those years. He goes oh my god.

Now when the Marine Corps was asked why they didn't provide the correct data whenever this e-mail was sent to them by Kelly Dreyer, who was the project manager for this thing, Tom Townsend, who is a retired major and lives in a cave out in Idaho -- he doesn't really live in a cave, but he likes to say that. He's like a hermit.

But he wrote over a thousand FOIAs. He lost a son and also his wife, and he was very diligent in writing Freedom of Information Act requests. And Tom Townsend identified this. And Tom Townsend you've got to understand, everything he writes, he does it by hand on a yellow legal pad, and that's his official correspondence. He don't type.

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He doesn't use a computer, and that's how he sends his stuff out.

The Marine Corps said they used, they saw that he had copied ATSDR on his initial letter pointing out this incorrect data. So they surmised that ATSDR was going to use his letter pointing out the wrong, the incorrect water system data as their notification. They said this in a press interview with Dan Rather and an AP article.

Well, you saw what kind of trouble ATSDR had on 2 September in 1994. Here's a letter from December 9th of 2005. "ATSDR has experienced delays in obtaining requested information and data pertaining to historical water-quality sampling data and site remedial investigation reports." And they were told.

"ATSDR staff is attempting to meet the project completion timelines discussed with Marine Corps staff in August. To do so, we must be provided all documents that relate to base-wide water issues immediately. The Marine Corps is responsible for the identification and timely sharing of all relevant documents relating to the base-wide

drinking water system. This includes documents that ATSDR may not be aware of as well as documents that are in possession of DOD but may no longer be located at the Camp Lejeune base. Discovery of this documentation must not rely on specific requests from our staff, but on our shared goal of ensuring scientific accuracy of our study and DOD's responsibility to provide the information. ATSDR staff can coordinate with the United States Marine Corps staff to determine the appropriateness of any document as it relates to our study. We request that your staff verify and confirm the existence of the documents listed in the attachment. We also request that your staff identify for us any other documents that may be useful to ATSDR for its water modeling analyses," and it goes on and on.

Yesterday we find out, we had our Community Assistance Panel meeting, that there's another whole file of documents related to underground and aboveground storage tanks, some electronic portal from a contractor. I mean, this never ends.

These are a few examples of the misinformation, disinformation, half-truths and outright lies that have been told by representatives of the Department of the Navy and the United States Marine Corps. There are many, many more. They have provided inaccurate data to the ATSDR, they have misrepresented the levels and the extent of the contamination to the media and to the public at large. They have, and they continue to misrepresent their negligent behavior which created the conditions that led to the drinking water contamination aboard the base.

Their negligent behavior was they just ignored it. They had warning after warning after warning. They were told by I don't know how many different analytical laboratories in I don't know how many analytical samples and results that they had a problem with these contaminants, and they never tested their wells. They never tested the individual drinking water wells until they started in July of 1984 knowing full-well they had a problem.

The Marine Corps' representative, who

did the interview for Dan Rather's story last October, was a Lieutenant Colonel Mike Tencate from Headquarters Marine Corps. He's a lawyer. He sat right there and told Mr. Rather that whenever they discovered that they had a problem with their wells, they took them offline. Mr. Rather asked him, he said where do you get your water? He said from wells. But you never tested them? You knew you had this stuff in your tap water, you never tested them? He repeated his answer again. Whenever we discovered that it was in the wells, we took them offline.

They tried to make the excuse that they thought they had AC-coated pipes that was creating this stuff in the water. Trouble is they never went back and even checked what the construction materials of their own water system was to verify or deny that claim. Morris, in his water modeling, has shown that there was only AC-coated pipes in one water system, and that was Holcomb Boulevard. The two highest contaminated systems had none in it, Tarawa Terrace and Hadnot Point.

And in my statement here it says in a

recent interview with Dan -- I already went over that. As soon as they discovered he said they took the wells offline. Well, the sole source for drinking water at Camp Lejeune are deep ground water wells. Exactly where did the authorities at Camp Lejeune think this contamination was coming from or emitting from. It wasn't coming from the supply wells. Perhaps they had some rogue water treatment plant operator at the treatment plant pumping these chemicals into their treated water, right?

The truth is that base officials knew about it by August of 1982 that the well fields for Tarawa Terrace and Hadnot Point were the source of the contamination aboard the base's water supply system. Instead of decisive action, excuses were made, the base supervisory chemist offered a suggestion that some of the contamination could be coming from asbestos coated pipes in the systems. Well, the only instances where any contamination was discovered in that system was when the base operators were opening in the clean Holcomb Boulevard system, was when the operators were

opening and closing the isolation valves which interconnected the Holcomb and Hadnot Point systems.

And, you know, there are some very pertinent questions which need to be asked here. Why didn't the Department of the Navy and USMC officials research the construction materials of the contaminated system back in the early 1980s? The main question would be why did it take more than four years to sample the supply wells? In that, that question has been asked multiple times and no one can get a straight answer from the Department of the Navy or the Marine Corps.

It was my understanding that this expert panel was requested by the Department of the Navy. It is my opinion that they are hoping that this forum will kill the Hadnot Point water system modeling. In fact, I believe they would like nothing more. If science is ever going to have a better understanding of the effects of these chemicals have on human beings, it is imperative that this effort continue. If the victims of this tragedy are ever going to

fully understand what they were exposed to or what caused the death of their loved ones or their illnesses, this water modeling effort must be seen through to its completion.

And my involvement in this is my daughter, Janie, was the only child of mine that was conceived while her mother and I lived at Camp Lejeune in one of the contaminated housing areas. When Janie was six years old, she was diagnosed with acute lymphocytic leukemia. I watched Janie go through hell for two and a half years before her ultimate death.

And from the date of her diagnosis until the date that I found out about the contamination, I did what any normal parent that had a child, who lost a child to a catastrophic long-term illness would do. I wondered why. And it was fourteen and a half years until I was walking in the living room with a plate of spaghetti to watch the evening news and the Public Health Assessment had come out. And one of the local TV stations picked up on the story and did a blurb on the evening news.

And I was -- I just walked into my chair. I was standing there and the reporter said the contaminants that have been found in Camp Lejeune's drinking water from 19 -- they erroneously said from 1968 through 1985 at that point -- were linked to childhood cancer, primarily leukemia. I dropped my plate of spaghetti on the living room floor, and it was like God had opened the sky up and said, Jerry, that nagging question that has been with you for fourteen and a half years, here is a possible answer to it, not a confirmed but a possible one.

And I started making phone calls and started digging. Here I am. That was August of 1997, and I've been asked when I'm going to give this up. And I've made the statement to the press and I made a statement indirectly to the Commandant of the Marine Corps. I said I'll give this up when you do what's right by our people or when you pat me in the face with a damn shovel and blow Taps over me, that's when I'm going to quit. And I mean it. Thank you.

DR. CLARK: Mr. Ensminger, we thank you for

your statement. Would you be willing to take some questions?

MR. ENSMINGER: Certainly.

DR. CLARK: Does the panel or anyone in the audience have any questions or comments?

MR. HARDING: Bob, I have some for Mr. Ensminger. I suspect I know the answer to this, but I'd like you to address it directly because one of the charges that we have is to ask if the timeline of this study is sufficient. And you've heard, you've been here the whole time. You've heard all of the discussions about the technical difficulties and the complexities of this and some discussion about whether it can be done by, what is it, December. And I wanted to know what you and also your sense of the rest of the stakeholders you're associated with think of a longer time to get an answer if the answer could be better.

MR. ENSMINGER: I, personally, and I know some people that said, you know, that there's been enough time spent. Those people aren't really as deeply involved in this, but anyone who is deeply involved -- and Mike Partain is

another victim back there.

He was born at Camp Lejeune. His father and mother lived there, and he was conceived there and born there. He ended up with being diagnosed with male breast cancer two years ago. We've also identified ten other cases of people at Camp Lejeune, either dependents or male Marines who had breast cancer.

But to answer your question, I know science takes time; good science does take time. And I have no qualms at all with taking more time to ensure a good product, and that's my answer.

DR. HILL: Just a quick question, the excerpt from CERCLA 47, do you have a year for that?

MR. ENSMINGER: A year? Yeah, it was May -- no, I'm sorry, August of 1988.

DR. HILL: Nineteen eighty-eight. Thank you.

DR. CLARK: Any more questions or comments from panel or audience?

(no response)

MR. ENSMINGER: Now, to go back to that

other question about how much time it's going to take. What I do take exception to is the dragging this thing out by the trickle of documents. And every time something new comes out it kicks this thing to the can further down the road, and that pisses me off. I mean, I should say it frustrates me. Dr. Sinks does not like some of my mannerisms. I'm me. I'm a retired former Marine. I was a drill instructor and I am what I am and you get what you see.

DR. CLARK: Anyone else have comments or thoughts, questions they'd like to raise for Mr. Ensminger?

MR. HARDING: I just have a comment to the panel. Just many of you may be aware of this, but there was a, if you will, an epidemic of TCE contamination events discovered in the fall of 1980, and I guess Bob might know this. I think it was a regulatory requirement at EPA that this testing for THMs be done.

And I've seen other documents just like this. And it, literally with the GC trace on it with an arrow saying, you know, possible TCE contamination. And this is how,

I know it was true in Phoenix. I think it was true in Redlands, California. I can't remember, a number of the cases that I've seen where this October of 1980, there's a lot of this that went on.

DR. CLARK: It turned out that when we were working on the THM methods that they were very good for capturing VOCs at the same time. And it was kind of a confounding and puzzling effect. But the point that Mr. Ensminger makes is absolutely valid. And I do have a question.

First, Mr. Ensminger, you identified correctly, I think, the fact that the THM samples had VOCs in them. Did you look at anything other than just the three samples that you --

MR. ENSMINGER: Oh, yeah, there's many more. I mean, there's, we've got a whole file of the TTHMs from the Army Environmental Hygiene team and then the Grainger Laboratory that wrote the letter. We understand that they were told by the Department of Navy to quit quantifying the amount of chemicals, the interfering chemicals, they were finding.

So they put on there by it with an asterisk that this chemical was still being found in that water system and tetrachloroethylene was still being found in the Tarawa Terrace system. They quite quantifying it, but the actual analytical results, there's many of them, and they're in the files.

DR. CLARK: Did you do any looking at samples at a given location over time, for example, after those wells had been taken offline to see if there'd been changes in the THM values?

MR. ENSMINGER: I really didn't see that many TTHM samples after the fact. I don't know. I haven't seen them. I'm sure they're somewhere.

DR. CLARK: They would be required to submit them to the state, but that's something --

MR. ENSMINGER: The State of North Carolina is like, you know.

MR. PARTAIN: Jerry, that had that TTHM problem, too, at the air station.

MR. ENSMINGER: Yeah, they had a problem over at the air station with TTHMs. They

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exceeded the MCLs at the air station. And they had salt water intrusion over there.

DR. CLARK: Probably brominated compound. It's probably getting brominated compound.

MR. ENSMINGER: Yeah, that's what it was.

DR. ASCHENGRAU: I just want to follow up with you or the ATSDR folks about that file that you said was, came to light yesterday.

MR. ENSMINGER: Yeah, Morris had that on one of his slides this morning.

DR. ASCHENGRAU: So has it been given to ATSDR for review to see if there's any useful information in it?

MR. FAYE: That's your call, Morris.

MR. MASLIA: Bob's punting to me. Actually, in a series of e-mail communications between Bob, myself and the Marine Corps we became aware of it the beginning week of March of this year. And we did ask, it's, as Jerry pointed out correctly, it's housed at a website, web portal, by a consultant to NAVFAC, Katlan Associates, Katlan Engineers.

We have been given a password and access to that. Bob initially downloaded over 100 documents. We have -- not pages,

documents some of which are hundreds of pages long -- and that's why I referred to it as information because we've done an initial catalogue of that. We've got that on an Excel file.

And that's when I was discussing earlier today that perhaps one way to use this in the most efficient manner as the universe of information is expanding and trying to stick on some timeline, whatever that may be or the panel recommends, would be to view this as a second, quote, independent set of data that we might cull from those documents. Develop a model, calibrate to a set that's already been described here that Rene and Bob and Barbara have described, and then perhaps be able to test or give ourselves more confidence on running the model with this second set.

That would do two things. One, it would not completely ignore this other data. It would keep us going down the path, but it would also answer questions that we, as people have pointed out that with Tarawa Terrace we did not have the opportunity to because the

data just weren't there as a second set of information. So that's thrown out.

Consider in your recommendations, if you would, for the panel members. But that's our thinking right now is that is a possibility. Obviously, you have do nothing with it, which I don't want to go down that road, or incorporate it with our current data, which we know how long we've been, what, since June of 2007, Bob?

MR. FAYE: Probably a year and a half.

MR. MASLIA: A year and a half already on data analysis and going through these documents and stuff like that. So if the panel would, I think we would appreciate some feedback on that.

DR. ASCHENGRAU: And then there's really no way of knowing right now if there are still yet other undiscovered sources of information?

MR. ENSMINGER: Well, we know that there's some key stuff that's missing from the files. I don't know if -- one thing I forgot to mention was that there's an Associated Press article out today, ATSDR withdrew the entire Camp Lejeune Public Health Assessment

yesterday.

DR. HILL: What does that mean?

MR. ENSMINGER: It's invalid. Benzene was left off of it. And we found, Mike Partain, who's my brain back there, he's been a godsend to me. We've been going through all these CERCLA documents and putting two-and-two together, and we discovered that the contractor that was doing the confirmation study at Camp Lejeune in 1984, in their plan of work and safety, work and safety plan for their contract in early 1984, agreed to a monthly progress report on their efforts to, on the confirmation study on all the contamination sites on the base to start in 1984.

We found the progress report for May, June and July. And in July the first samples were taken of monitoring wells and water supply wells that were close to the contamination sites. Oddly enough, we don't have any more progress reports for that confirmation study. They ended at July. So when they would have got started getting the results back, the August, September, October,

November reports, they're missing from the files.

But we did find a report of the analytical data. We can't even find the confirmation study report. The Marine Corps absolutely refused, they disagreed with the conclusions. I've got this in writing. And absolutely refused to release that report to any outside agency, but they did agree to release the analytical data.

We found the results from the July sample from Well 602, which was right by the Hadnot Point fuel farm, and it had high levels of benzene in it in July. Do you know when the well was taken offline? 30 November. You can't tell me this company didn't alert them that they had high levels of benzene in that well when they found it in that analytical result. That's why we can't find the progress reports for August, September, October, and November.

DR. ASCHENGRAU: So I do think it does fall within our purview to make a recommendation that all of the relevant information should be given to the research group and that would

affect our other recommendations for the modeling, et cetera.

MR. ENSMINGER: That would be appreciated.

DR. CLARK: Morris wants to say something.

MR. MASLIA: Yeah, I want to clarify for those who are on the panel who are not really familiar with the Health Assessment process. What Jerry just mentioned that the Health Assessment for Camp Lejeune, it's the 1997 Health Assessment, was pulled.

In a series of discussions, as Jerry said, one of the factors were -- and this is in one of the tables, I think Table 8 or C-8, C-10 in Bob's report -- you'll see benzene levels 720, 380 and so forth. That was completely omitted from the Health Assessment. That's point one. Yet, a year later, the 1998 Health Study coming out of Frank's division, mentioned benzene contamination of 700. So obviously, the data was not put into the Health Assessment.

Other issues, as have been pointed out previously, was the start-up date with the Holcomb Boulevard plant was incorrect. There have also been issues of, I guess when ATSDR

was moving offices, some of the original references to support the Health Assessment cannot be located.

MR. ENSMINGER: Not some, all. They can't even provide the supporting documentation for the thing that created the document. How in the hell can you make a stand, stand on a document and stand behind it when you don't have the supporting documents that it was created from? It's worthless.

MR. MASLIA: As a consequence, yesterday our Division Director and Tom Sinks told the CAP that the Health Assessment, the 1997 Health Assessment, was being removed from the website. It's still, as any document would be, in hard copy if someone requests it. But if they request it there'll be a caveat or some letter with it explaining that.

And, of course, then they would wait until we finish the current study investigation for Tarawa Terrace and then also the Hadnot, Holcomb Bridge area to do whatever Agency management decides what approach they want to take. So I just wanted to clarify that for those who are not familiar or with

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the Health Assessment itself.

DR. CLARK: Walter, you wanted to make a comment?

DR. GRAYMAN: Yeah, this morning there was at some point, there was a graph shown in which it showed that there's a lot more data available from 1998 to the present time. And the explanation was that, and I can't remember whether it was federal or state law regulations that the utility hold onto the records for ten years. Is there something that can be done to ensure that that period is extended so we don't start losing data that becomes ten years old and then is lost?

DR. CLARK: I'm assuming that that's probably a state agreement in conjunction with EPA, but I don't know that.

MR. ENSMINGER: It's a CERCLA requirement. And it's required to be maintained for 50 years on any site that's declared a ~~super fund~~ [Superfund -ed.] site. And there's all kinds of stuff from Camp Lejeune missing. Now they keep saying they have this seven year, in-house requirement to purge their files. I hate to tell them, but they're in violation of

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the CERCLA laws.

And, you know, Morris and Bob Faye had an experience up at the State of North Carolina's archives when they were trying to find all the operating permits for the water system at Camp Lejeune. And they went in there, and they found everything from the beginning of the base, to the opening up of all the different water treatment plants, the water distribution systems, and it went from 1941 to all the way up to, what, 1968, or no, '68? And then from '68 all the way to 1990 or '91, the file folder was there. Everything was gone. And then from that point to present everything was there. You tell me.

DR. CLARK: Any more questions of Mr. Ensminger?

(no response)

DR. CLARK: Comments?

(no response)

DR. CLARK: Well, thank you very much for your presentation. I think --

DR. CLAPP: I was just going to say the same thing the Chair just said. I'd like to thank Jerry for his service and his presentation.

DR. CLARK: Well, I think he reminds us that there's a human dimension to this study that we have to keep in mind. I think we, it's very easy, as you can, if you remember from the previous discussions today, to get lost in the science and the wonders of that aspect of what we're doing. And we'll have more of that tomorrow, but there's a human, real tragedy in some sense, involved in this situation.

MR. ENSMINGER: We have a website we created for the victims of this thing, and it's www.TFTPTF, that's the abbreviations for The Few, The Proud, The Forgotten-dot-com. And I'm going to tell you, people contact me all the time. You would not believe the cases of non-Hodgkins lymphoma, the cases of leukemia, liver cancer, kidney cancer, bladder cancers of former Marines and sailors and their family members that are coming to our website.

It's horrible, and I'm fearful, when we finally do find out the truth in this thing, when we uncover it, we're going to be uncovering one grave at a time. I hope not, but I believe that's what's coming. And I have one more thing to say. You saw the

examples of the lies. You've got them right there in your hands. There's only one reason to lie, and that's because you're guilty.

MR. PARTAIN: I'd also like to invite the members of the panel, on the website there is a historical timeline of events that's referenced with actual documents. Most of them are available on the website. We can pull a document up and read that. It's under the historical document section.

It's rather long boring reading, but it at least gives you an idea of what happened. And that goes from basically 1950 to 1989, and I'm currently working on the second half of that project, 1990 to the present day. And there's also on the discussion board on the website there is a discussion called Betrayal of Trust and Honor, which is an historical discussion.

My degree's in history -- I'm a former teacher -- you'll see I can read the stuff. And it's all referenced to historical documents, too, and that will give you an idea of what was going on. Jerry mentioned in his presentation about Cheryl Barnett saying that

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we didn't know until this study. Well, the study she's referring to is the confirmation study of 1984.

DR. CLARK: Thank you very much.

DR. GOVINDARAJU: Actually, could you please repeat that website again? I wrote it down.

MR. PARTAIN: It's The Few, The Proud, The Forgotten. If you take the initials, Tango, Frank, Tango, Peter, Tango, Frank-dot-com, TFTPTF.com.

DR. CLARK: Mary.

DR. HILL: So there's been mention of health effects that are further along in life than some of the ones that are formally being considered here. And I assume there was some investigation into those and there wasn't enough data to support that, but I just wanted to -

DR. BOVE: No, no, no, no. That's our future studies, which we can talk about at some point if we -

DR. CLARK: I suspect we'll end up discussing that further on as we get further into the discussion. I have the same reaction that you do.

Any more comments, questions on this particular, on Mr. Ensminger's presentation?

(no response)

DR. CLARK: Okay, to continue on --

MR. HARDING: Bob, just a comment on what Frank said and Mr. Ensminger, I wasn't completely clear that there were going to be follow-on studies, but it just raises the point again that this, that the key to all of that is going to be the exposure information. And so it's important that that be done as well as it can be. And I want to encourage, and this will be something I advocate in the panel, that ATSDR really focus its efforts on the things and maybe we can help them do that, that are most important to getting that information.

DR. CLARK: Very good comment.

Anything else?

(no response)

REPRESENTATIVE OF DEPARTMENT OF NAVY

DR. CLARK: We'll let Mr. Dan Waddill from the Department of the Navy ~~to~~ [-ed.]continue and I guess conclude our public discussion.

DR. WADDILL: Well, my name is Dan Waddill

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and I'd like to thank you all and ATSDR for this opportunity to address this expert panel. I work in the Navy's environmental clean up program as the head of the Engineering Support Section at NAVFAC Atlantic. My group provides technical support for Navy and Marine Corps sites across the continental United States and Alaska.

My educational background is in modeling of groundwater flow and contaminant transport, and I've been involved in numerous applications of these models at sites, Navy and Marine Corps sites. Last year I contributed to Navy comments on the ATSDR water modeling report for Tarawa Terrace, and I believe you have copies of those comments and responses.

I would like to say that the Navy and Marine Corps fully support the scientific effort to determine exposure concentrations and their effects at Camp Lejeune, and in particular, we support the work of this expert panel, and we do thank you for your efforts. As you move forward with your discussions today and tomorrow, I'd like to ask you to

consider three issues related to the groundwater modeling efforts.

But before I do that I'd like to explain how I'll use the words accuracy and precision in my comments because I think that will help clarify what I'm talking about. In the way that I'll use it accuracy is the extent of agreement between model output and measured data, and accuracy would be estimated by comparing the model to the real world.

For example, at Tarawa Terrace we would compare model-simulated PCE concentrations with measured PCE concentrations and that would give us a sense of model accuracy. Precision is the extent of agreement among various model runs, so precision would be estimated by comparing one model run to another as we do, for example, during Monte Carlo analysis.

So to get to the first issue in the existing charge to the expert panel, Section 2B asks which modeling methods do panel members recommend ATSDR use in providing reliable monthly mean concentration results for exposure calculations. And we certainly

think that is a good question for you to consider.

In addition to that I'd like you to consider a more preliminary question which is, or issue, which is whether or not modeling at Hadnot Point is capable of providing reliable average concentrations on a month-by-month basis. And in other words can we expect the model to distinguish concentrations from one month to the next with a degree of accuracy that would be useful for the epidemiological study or is monthly simply too fine a resolution for the model to achieve.

And why do I ask you to consider this issue? Well, we know that the modeling efforts at Tarawa Terrace and Hadnot Point both face a fundamental difficulty caused by the limited availability of real-world concentrations. The models are being asked to reconstruct historical concentrations back to the '40s or '50s, but prior to the 1980s there are no measured concentrations of PCE, TCE and the other contaminants.

For Tarawa Terrace ATSDR determined, and the Navy concurs, that there is not enough

measured PCE data for a meaningful model verification step. And since measured PCE concentrations are available only in the 1980s, model output from the late '70s or early '80s back to the 1950s cannot be compared to actual PCE data.

And we know that we have to ask the model to fill in data gaps. If we had enough measured data, we wouldn't need to model at all. We'd just use the measured data. But the question is, is 30 years, is that too big of a gap to be filled in by a model on a month-by-month basis.

To evaluate model uncertainty probabilistic analysis was used at Tarawa Terrace, numerous model runs compared against each other. So that gives an idea of model precision and the uncertainty based on model precision. And this is good information. It's a standard modeling technique, standard approach. And it gives us a sense of how tightly clustered that model output is. But it doesn't necessarily tell us if that cluster of output is centered around the real result. Is it hitting the real-world target?

For Hadnot Point the situation is similar in that the model would need to extrapolate concentrations back in time over roughly 30-to-40 years. As we've discussed already, the overall situation at Hadnot Point is that it's significantly larger and more complicated than Tarawa Terrace was.

So the second issue I'd like to look more closely at model uncertainty, as I mentioned before at Tarawa Terrace, probabilistic analysis was used to examine uncertainty with respect to model precision. And this work occurs in the model world. I would also like to examine how the model compares to the real world and that would help us better understand uncertainty with respect to model accuracy.

And obviously there are long stretches of time without real-world concentrations, you know, they're just not available for comparison. But we do have those in the 1980s, and those comparisons were made for the Tarawa Terrace model during calibration. So that degree of fit that was attained during the model calibration gives us a sense of the

uncertainty that we might expect with respect to accuracy of the model.

For the earlier decades when we can't compare the model to real-world concentrations that accuracy is somewhat unknown, and I guess I would ask you to consider whether we would think the model would be more accurate in those earlier years than it was in the '80s or might it be similar.

And so just to sum up, I think it's important to consider the model precision, model accuracy, and to consider how the uncertainty in the accuracy can be assessed and conveyed to the model users. That would include the public as well as the epidemiologists.

Just as an example, you know, this morning when Dr. Bove showed the table of monthly model-derived exposures, the panel, you all asked, commented on the three significant figures. And there's a comment that it might be appropriate to show a range of values instead of a single value. And I certainly think that these are good suggestions, and it would be helpful to know

what that range would be as we move forward.

And just as an illustration, and I'm picking these numbers out of the air, if we have a value of 90 micrograms per liter, does that fall within a range of 60 to 150 or is the range more like 30 to 300 or is it 10 to 1,000. It would just be useful to have this kind of information passed along to the users of the model.

And the third issue is related to the second one. I'd like to look more closely at model calibration. The existing charge to the panel asks whether there are established guidelines for applying calibration targets and what the calibration targets ought to be, and again, I think this is very useful and appropriate.

Given that approach though I'd like to ask the panel to consider also how the model results ought to be interpreted when the calibration targets aren't met. And maybe that's not a good way of asking that question.

I thought perhaps a better way and a more general and useful way to ask that question would be simply how do we assess and

convey to model users the performance of the model during the calibration process. And I think this is important because it will shed light on model accuracy and the uncertainty associated with accuracy.

So just to sum up I'm asking the panel to consider three issues. First, given the limited availability of measured concentrations and the site-related difficulties and uncertainties that we've talked about, would modeling at Hadnot Point be capable of providing reliable average concentrations on a month-by-month basis?

And second, in addition to considering uncertainty with respect to model precision, how should uncertainty with respect to model accuracy be assessed and conveyed to the model users?

And third, how do we assess and convey the performance of the model during calibration? And issues really two and three could really be lumped together into one main concern that would be that model users be given a clear understanding of the model uncertainty.

And, you know, I've been working with Camp Lejeune for a year and a half or two maybe, so I certainly don't understand all the issues associated with it. But I can say that the Navy goal for this expert panel is simply to get your best recommendations for the best science that could come out of this result. And I know that you have a difficult job. This is a difficult site, and we certainly thank you for your efforts.

DR. CLARK: Dr. Waddill, would you be willing to take a few questions?

DR. WADDILL: Yes.

DR. CLARK: Do we have questions from the panel for Dr. Waddill?

DR. GRAYMAN: It's more a comment than a question. One danger when you talk about ranges for values is if the perception is that that range, that every point within that range is equally likely, and I would suggest maybe rather than a range of values, a likely distribution of what the values are going to be so the points at the end are probably less likely than the ones nearer the middle.

DR. WADDILL: I would agree with that and

really, I'm not asking you to, I'm just asking you what sort of recommendations might you have. I'm not trying to endorse a range.

DR. CLARK: Do we have any more? Mary.

DR. HILL: Just one thing. In talking about model fit, it's not true that just a really, if I was given, if I gave you a model that fit the data exactly, I would expect you to be suspicious.

DR. WADDILL: Right.

DR. HILL: So there's a balance there that's not always easy to deal with ~~and certainly~~ [uncertainty -ed.] from your position.

DR. WADDILL: I agree. I agree with you completely.

DR. CLARK: Do we have any more comments from the panel or –

MR. HARDING: Yeah, sort of along those lines it's common to view analytical results as the truth, as the true value. But in fact, they are only an estimate of the true value, and what that value is depends on the question that's asked. And the model's being asked a slightly different question because we're dealing with a month-long stress period.

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Somebody walks out with a sample bottle and takes a sample out of a well. And as I think Mr. Faye, Dr. Faye talked about the fact that things can change pretty fast under pumping regimes. We've seen cases where they'll change two orders of magnitude over a period of a couple of weeks of pumping.

And so I think it's really important as you think about that if you have a value that doesn't agree, so it affects your definition of accuracy, you really have to look at that in a much more, in a much richer way, a much deeper before you decide whether that's really saying the model isn't performing the way it should.

DR. WADDILL: Yeah, I agree, and I really just, you know, there are all kinds of issues associated with sampling and analysis, and there are inaccuracies associated with that, too. I just think that what I'm asking is that you consider the comparisons to the real-world samples that we have and to address among yourselves what's the best way to assess uncertainty. And I didn't mean to imply that I have an answer for that. That's a tough

one, and I'm just asking you to consider it.

DR. CLARK: Do we have any more –

DR. GRAYMAN: Bob, just an add-on to what Ben says is that when you start going into distribution systems and look at water quality, you can have changes literally within minutes because of the dynamics. I could very much see this being the case in Holcomb Boulevard where you take the sample, and it reads something. And ten minutes later you took another sample, and it may be absolutely, totally different. So you have to be very careful in distribution systems.

DR. CLARK: Do we have any more? Richard.

DR. CLAPP: Just one more time. Dr. Bove said this morning I think the National Academy of Sciences Report, which has been delayed, will say the same thing, which is that we're not actually looking for numerical values for each individual subject. We're looking for a ranking of those, and just to make that point again.

DR. HILL: I have a question. Oh, go ahead.

DR. ROSS: Along those lines and for clarification of folks like me without much

epi background, there's a response to the Don's comments that reads if I could just humor me for a second. I'll bore you.

A successful epidemiological study places little emphasis on the actual-parentheses-absolute estimate of concentration, and rather emphasizes the relative level of exposure. Can you enlighten me? And this speaks to the objectives of the model. What the objectives are.

DR. CLAPP: Well, I don't know how to say it more clearly than that actually. It is, for each individual subject, and that's like I said, for example, a child with a birth defect or a control in that study or later on in a person who died of kidney cancer versus a person who was at the base but didn't die of that.

We're looking to see whether in a relative scale, the exposed people were more likely to have gotten the disease, and so it can be -- for example in Woburn, in my own work on Woburn, we were looking at categories highly exposed, moderately exposed and either not exposed at all or unexposed. And we saw

it. We actually saw that result that the highly exposed were much more likely, in my first study ten times more likely, to have been diagnosed with childhood leukemia than the controls, so in that stratum of highly exposed.

So it's really not about that you have to have had a cumulative lifetime exposure of 500 parts per billion or 531 parts per billion versus 497 parts per billion. It's are you in the high exposed, the medium exposed or low exposed. And that's how most of these studies are done. And especially in a situation like this where the data are either going to be uncertain or sparse. That's the best we can do.

DR. WARTENBERG: Just to follow up on that, the methodology that's used for those, the analysis Dick's talking about, look at if one goes up is that associated with a greater likelihood of disease. So it doesn't really use the numbers. You can back out of some of the numbers to try and have a handle to talk about it. But, in fact, the analysis doesn't care if the numbers are from one to ten or

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from one to a thousand. It still looks for that association. And that's why the comment is don't worry about the numbers. That's not the point of the analysis.

DR. WADDILL: I guess as long as the model is accurate enough to get the trend right and the ranking right, that would be my understanding.

DR. WARTENBERG: Where it becomes trickier is when you start grouping the data, I mean, what Dick was saying about having different categories, then that also becomes sort of tricky in terms of either making clear what the association is, but if it's done some ways, it can also make it more obscure.

DR. CLAPP: And luckily we have an expert on how to do those cut points sitting right here.

DR. HILL: So if I consider a first order analysis to be take the existing data I have at these different wells, and just assume, from that get some average concentration for those wells over time, and then apply the pumping schedule, I would get exposure rates for different communities, and they could be fit into these different categories. That

would just be a first order.

Okay, so the question becomes in what ways can we use a groundwater model to improve on that first order estimate. Is that a rational --

DR. CLAPP: That's what I think we're doing here, yes.

DR. HILL: Has that first order analysis ever been done?

DR. CLAPP: Not yet, but I mean for example for Tarawa Terrace, that is now available to do that. It needs to be --

DR. HILL: Right, for either the numerical modeling or this first order analysis, you have to figure out some pumping schedule, but that's a step that's in common to both of them.

DR. CLAPP: Yeah.

DR. HILL: So it's just, it seems to me like that's the framework I'm thinking of in terms of --

DR. CLARK: Frank, did you have a comment?

DR. BOVE: No.

DR. CLARK: Do we have any more comments or thoughts for Dr. Waddill while we have him

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here?

(no response)

DR. CLARK: Thank you very much. We appreciate your coming in, sir, very relevant, very important and good advice to the panel. Thank you.

MR. MASLIA: We can hook Scott up. We'll take a ten minute break?

DR. BAIR: I'm a lot more nervous about this than I was an hour ago.

MR. MASLIA: Take a minute break while we hook you up. So if we can start back at five o'clock.

(Whereupon, a break was taken between 4:50 p.m. and 5:00 p.m.)

DR. CLARK: I guess they've been live video streaming all through this break so time to get back on board and get going. Scott's going to talk about some of his studies at Woburn, which I think would be very informative and useful for our discussion.

(Whereupon, a presentation was made by Dr. Scott Bair from 5:00 p.m. to 6:00 p.m. The meeting concluded for the day at 6:00 p.m.)

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I, Steven Ray Green, Certified Merit Court Reporter, do hereby certify that I reported the above and foregoing on the day of April 29, 2009; and it is a true and accurate transcript of the testimony captioned herein.

I further certify that I am neither kin nor counsel to any of the parties herein, nor have any interest in the cause named herein.

WITNESS my hand and official seal this the 19th day of June, 2009.

Steven Ray Green, CCR

STEVEN RAY GREEN, CCR, CVR-CM, PNSC

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